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TONAL ATTRIBUTES AND FREQUENCY THEORIES OF HEARING

BY RALPH GUNDLACH

University of Washington

Of the numerous adjectives which have been used to describe tones, three or four seem to denote distinct, independently variable, and fairly exhaustive attributes (Ogden, *13*, p. 71). Disagreement arises not so much with respect to pitch and intensity as to volume and brightness.[1] Physicists usually fail to recognize B except as differences in overtones, and ignore V. Several psychologists class V and B together as vocality, or tonality. Banister would accept B, but discards V as applying merely to the imaginal size of the sound-source (*2*), while Boring ignores B, but erects on V his theory of localization (3). Similarly, we find disagreements as to the physical dependencies of the attributes; Max Meyer (*11* and *12*) and Boring (*3*), assert in the interests of their auditory theories that I is correlated with the amplitude of the sound wave, rather than with the energy; and Boring contends further that V is dependent upon the amplitude only, and is quite independent of P. Some settlement based on experimental data should be reached with regard to these attributes.

This paper attempts (1) to describe briefly how these four attributes have been, or may be, demonstrated; (2) to discuss Abraham's explanation of the basis of B (*1*); (3) and (4) to consider the relation of I and V to the energy and amplitude

[1] Attributes will hereinafter be referred to by the initials, P, V, B, and I.

of the wave, with special reference to auditory theory; and (5) to discuss V as an attribute.

1. *Demonstration of tonal attributes.* Two pieces of apparatus as sound sources are needed for demonstrating the auditory attributes. The one should vary the sounds in frequency and intensity. Energy remaining constant, a change in the frequency will result (*14, 4*) in a change in P, V, B (and, it may be, in I). With the frequency constant, however, a change in energy modifies V and I, B and P remaining constant (*6*). It is thus shown that B and V are independent. The other sound source may be a siren disc so arranged that in each annular series there are the same number of holes, the difference between the sets consisting of the relative amount of space between the separate holes (*1*). At any given speed of rotation, then, the frequency of every set will be the same. With constant energy, a change from one set of holes to another involves predominantly a change in B, the tones from the outside being brighter, those inside being dull. But there are also differences in V and in I. The tones from the outside are less voluminous and more intense than the tones from the inside of the disc. Again, a change in energy at one level of holes chiefly changes I.

The changes are thus seen to be quite complex. Two or three aspects seem to change at once; a change in I changes V; a change in B changes V and I; a change in P changes B and V. But it is clear that if the demonstrations are correct, there are four separable aspects of tones.

2. *Brightness.*[2] In puzzling over the siren disc several interesting problems arose which changed our conception of B. How, for instance, would impulses such as arise from the disc affect a system of resonators? It would seem that a shortening of the duration of the impulsion (wave length remaining the same) should have little or no effect upon the quality of the fundamental. Yet we find very marked changes which Abraham will not attribute to overtones (*1*). Again, the most efficient result should come from the impulsions

[2] Professor Bentley's interest and suggestions are chiefly responsible for this section.

most evenly balanced: where the wave best approaches a sine curve. That is, the set of holes near the center, where equal hole and space alternate, should be relatively more efficient than that set near the periphery, where between each pair of holes there extends some ten times as much surface. But the tone from the inner circle is not very strong; it is feeble, quite breathy and dull, while the tone from the outer edge is hard and compact, bright and loud. Might not the differences in B (and loudness) be due to some other factor in the situation, *e.g.* to the differences in the speed of the rotating disc as it passes the air column? To test this possibility three other discs were tried; the first solid (no perforations) which gave from center to periphery no differences in sound; the second a disc which had one hole only in each of the concentric circles, and which again gave very marked differences in B although the P was almost indiscernible; and a third disc of somewhat different construction. This disc was divided into four parts by the intersection at right angles of two diameters. Two sectors on opposite sides were cut out; the other two quadrants were left as solid sectors of 90° each. Thus at any distance from the center the air jet would meet alternate open and closed sectors of like extent (90°). The form of an air wave intersected near the center or near the periphery would have no distorted proportions of crest and trough, except for the differences resulting from the different speeds at which the various parts of the disc pass the air-source. The conditions Abraham thought necessary for B were not present, yet differences in B were as marked under these conditions as with the disc which had the various spacings between the holes.

To determine whether the distortion due to relations of hole and surface had any effect on B, a large cylinder was constructed in which long triangular cuts were made. The base of the triangles was as wide as the distance between them. When the cylinder was rotated about its axis every part of the surface would move at the same speed. The apparatus was crude, but it was obvious that the slight differences in the tones produced even under the extremes of these con-

ditions were not of the same character as the B differences produced by the rotating discs. It may be, then, that differences in B of sounds of the same fundamental frequency are chiefly dependent upon overtones.

3. *Intensity.* The relation of I to the physical stimulus was brought out in Boring's theoretical paper. He suggests that "if intensity depends upon the number of cochlear fibers stimulated, all functioning under the all-or-nothing principle, then amplitude of vibration, rather than energy, would be the stimulus-correlate of tonal intensity" (*3*, 162). Again, "The direct correlate of auditory intensity would be the amplitude of the tonal stimulus and not the energy" (186). This is likewise Meyer's view when he says that "the hydraulic theory makes *sensation intensity dependent on the number of sensitive cells stimulated*" (*12*, 260 f.). The determination of the dependence of I appears crucial for Boring's and Meyer's theories. The experimental work necessary for an answer to the problem was reported in 1922 by MacKenzie who published a masterly experiment which gave the relative sensitivity of the ear at different levels of loudness for 10 different frequencies between 100 and 4000 d.v. (*10*). He reports that if a tone *a* is equated in I with a tone *b*, and *b* with *c*, then *a* is as loud as *c*. This addition holds for any number of stages, and is thus an excellent index of the reliability of the method. A tone of 200 d.v. was equated with one of 3000 d.v. both directly and by this method of consecutive equation. In this case the ratio of the energies is 200 : 3000 : : 25.7 (24 by the indirect method) : 1. The ratio of the amplitudes would be 200 : 3000 : : 76 (or 69) : 1. He also presents a table, averaging the results of 20 observers, in which constants are given for determining the ratios of the energies of various frequencies which would balance a certain pressure (energy) at 700 d.v. Using his formula

$$\text{Log Pf} = \text{A} + \text{B Log Pf}_2$$

where Pf is the pressure at that frequency and the values of A and B are given in the table, the ratios of the amplitudes of the tones to one of equal loudness at a frequency of 700

(pressure at 1 and 8) have been computed. These are listed in Table I.

TABLE I

THE RATIO OF THE ENERGY AND AMPLITUDE OF VARIOUS FREQUENCIES TO A STANDARD OF 700 D.V., WHEN EQUAL IN INTENSITY (LOUDNESS).

RATIOS FOR TWO LEVELS OF LOUDNESS

(Data computed from MacKenzie, *10*)

	First Intensity		Second Intensity	
	Proportional		Proportional	
Frequency	Energy	Amplitude	Energy	Amplitude
200	5.76	8.40	43.24	8.134
300	3.32	4.24	20.19	3.697
500	1.62	1.78	12.47	1.746
700	1.00	1.00	8.00	1.000
1000	.617	.549	4.93	.549
1500	.355	.277	2.78	.275
2000	.251	.175	1.60	.166
3000	.151	.090	.868	.077
4000	.072	.047	.337	.036

From this table it may be seen, *e.g.* that a tone of 200 cycles matched in intensity to one of 700 must have nearly 6 times its energy and 8 times its amplitude. The other ratios may likewise be seen at once.

More recently Steinburg has computed a formula based on other evidence showing that the loudness of a sound is a function of its energy-frequency-spectrum, and its level of loudness above the threshold of hearing (*15*). Finally, Kingsbury has determined the contours of the Fletcher-Wegel figure for the relative loudness of pure tones from 60 to 4000 cycles, taking as standards tones of 700 d.v., 10 threshold units of I apart (*8*). These three reports demonstrate that intensity is not proportional to the amplitude of the sound-wave.

Meyer, in considering Kingsbury's work, holds that these results are "unrelated to any physiological theory of audition other than the hydraulic theory" (*12*, 263). His explanation seems to be that due to the viscosity of the cochlear fluid there would be some discrepancy between the amplitude of the tone and the length of the "phragma" affected, and that this discrepancy increases with the increase in frequency. We should infer from this that, with amplitude constant, a high tone would affect fewer sensitive cells than a low tone

(*i.e.* would be weaker); and this is contrary to the facts. But if our interpretation of Meyer is wrong, as he says is the case with most people, even the reciprocal of this interpretation would not be able to explain such enormous differences in energy and amplitude of air waves differing in frequency yet reported equal in intensity.

4. *Volume.* The independence of V and amplitude may also be shown from other experimental sources. Halverson, keeping P constant and varying energy, found both I and V to vary, although the limens were quite distinct, and that for V was much the larger (*6*). V was established by Rich, however, as a covariant of P (*14*). Boring has argued that the dependence upon P is an artifact, and that V actually varies only with amplitude (*3*). Now, if Rich's V limens were based upon differences in amplitude, and amplitude is also the basis for I, then one should be able to obtain an I limen smaller than Rich's V limen, under his conditions. The writer previously found no I limen within the limits of the V limens,[3] when frequency was varied (*4*), and has verified this with four more observers.

But we need not rest with this indirect evidence. In a set of experiments carried out in Urbana, a group of subjects trained as above (p. 188) equated pairs of unlike frequencies both for I and V. Since the apparatus was much cruder than that of the physicists, a comparison with their results for I affords a check upon the accuracy of the present work.

The sound source was an oscillator previously described (*4*). The set-up was so modified that throwing a single switch would make a sufficient change in capacity to give the new frequency. A variable resistance across the phone circuit made rapid alterations in intensity possible. The frequencies compared were 100–400, 400–800, 800–2000. The energy for one frequency was left constant, and the energy of the other raised or lowered until the two were declared equal in intensity. This point was noted, and the increase

[3] There were 10 observers, Drs. Culler, Higginson, Guilford, Sullivan and Möller of the instructional staff: and graduate students McDonald (who observed for Halverson), Fox, Zuschke, Bidwell and Ireland, at the University of Illinois.

continued until the variable was declared louder than the fixed frequency. The energy input to the Baldwin phones was measured by a sensitive thermocouple and a galvanometer. It was almost impossible to get an equation for V for the different tones. With the phones on the head the higher tone would soon be so intense as to be painful, and cause violent starts and mutterings on the part of all the observers. If now the higher tone was kept just below the point of intensity which would drive the observer out of the room, the lower tone could be reduced below its standard. But the V of the lower tone would not lessen below a certain point; the boundaries of the weak tone appeared even more and more indefinite. Finally, if such a weak tone was equated with the loud higher one, there was not usually enough current to measure on our galvanometer. Consequently some of the determinations were made with the phones off the head and at a fixed distance from the observer.

On the first day of experimentation after training, none of the observers would declare the tones equal in V.[4] The experimenter insisted on their returning, however, for further attempts at reaching equality. Some of the observers did finally make judgments of equality, but the situation was comparable to a "second degree." The report of equal may have been in part unintentionally aimed at stopping the painfully loud noises. One observer, in fact, at first refused to come back for a further series, saying the previous sitting had made her hysterical.

Some of the comments made during the series, although chiefly figurative, indicate the observer's opinion about V and I.

The softer seems more spread-out; the other, no matter how intense, seems to fill a smaller space. I don't think I changes the V much; the more intense tone seems to shoot out in a straight line, without spread. (M. S.)

The tones never could be equal in V. The lower one has a fringe, a diaphanous border which the more precise high tone cannot reach. (L. S.)

The lower tone fills up more space, although the upper is much more intense. (N. F.)

[4] The writer is obligated to Miss Steffy, Miss Stevens, Mr. Firkins and Mr. Zuschke (University of Illinois) for their generous service as observers.

It does no good to change the I; it is like trying to make a needle point swell to the size of a drop of ink on blotting paper by pushing the needle in farther. The little one just yells from one spot. (W. Z.)

For the few equations that were forced out of the subjects the relations of amplitude have been roughly determined.[5] Our data include the equations in energy for tones equal in I and V. From Table I we may interpolate the relative amplitudes of tones equal in intensity. The sq. root of the proportional increase in current, in one of the two tones equal in I, necessary to equate them for V will give the relative increase in amplitude. Thus we find:

	100–400	400–800	800–1000
The ratio of ampl. of tones equal in intensity	8.6 : 1	3.5 : 1	4.6 : 1
Av. relative increase of ampl. required for equal volume	: 3.9	: 4.2	: 2.1
Range	2.8–4.84	2.0–6.0	1.4–3.3

Equality was reached in two cases when the amplitude of the louder, higher sound was less than half that of the lower; and in the other case, the amplitude of the upper tone was considerably greater than the lower. V is not, then, dependent upon the amplitude of the stimulus.

These judgments of equality for V are not, however, beyond question. None of them were made on the first day of the reporting, for then all the observers declared equality was impossible. Nor are any of these judgments of equality made when the phones were on the head; but when the sound came from some distance. Since the observers were requested to return to this painful situation and make judgments of equality, it is doubtful whether the reports were entirely unforced. Furthermore, the great variability of the amplitude of the higher sound casts additional doubts on their significance. While a just-noticeable increase in I requires a change of .056 in the amplitude of the wave (9), the range here is 2 or 3 times the amplitude of the weakest tone equated

[5] The writer left Urbana before the phones could be calibrated, so their sensitivity at various frequencies cannot be stated. At a given frequency, however, the output is proportional to the energy. The actual figures are therefore bare approximations; their value lies in showing the enormous variability of the report "equal in V" under these conditions.

in V! It may be noted that the energy is increased by the square of this amount. V may thus not only be independent of the amplitude of the air waves, but, since tones of different frequency cannot be equated in V, independent of changes in I as well.

5. *Discussion.* The reports on V bring it again in question as an attribute. We have found that it varies inversely with B; but, contrary to Halverson's results (*6*), we fail to discover its relation to I. In fact, it may also be remembered that on the siren disc the relation between V and I is quite the reverse; the more voluminous (dull) tones were the less intense; and finally, the limens for V (ten observers) with a change in frequency (*4*) are not comparable with those obtained by Rich (*14*). Perhaps it would be well if we could throw V out, as Banister has desired (*2*); although certainly not for his reasons (*4*).

It may be that, in the complexity of tonal attributes, V has originated from confusions both with I and B, as well as with the visual image. The observers, other than the dozen or so the writer has trained, who have reported on V have not been trained to report on B by means of the siren disc (*5*, *6*, *7*, *14*); and the observers so trained on B have said that V and B were reciprocals (*4*). If these observers are right, then these two terms might be dispensed with in favor of one or the other. B and V are found independent only when V seems to vary with I. But under the conditions of the experiment reported in (4) the V of one frequency did not seem subject to large fluctuations, irrespective of the I changes. That is characteristic likewise of B. Again, I does not vary with small changes in frequency, but V and B do. And, finally, on the siren disc the intense tones have the least V (most B). There is not at present sufficient evidence for two unlike attributes of tones in addition to P and I.

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(Manuscript received August 30, 1928)

THE RELATIVE POTENCY OF COLOR AND FORM PERCEPTION AT VARIOUS AGES

BY CLARA R. BRIAN AND FLORENCE L. GOODENOUGH

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The purpose of this experiment [1] was to determine whether the perception of color or the perception of form plays a major part in the total apprehension of an object by individuals of a given age and whether this relative potency tends to change with age. The method employed was a modification of that used in a similar study carried out by Descoudres [2] and published in 1914. It involves the setting up of an experimental situation in which the subjects are required to choose between two alternatives in matching a series of objects. When the objects chosen were similar in form, they were invariably dissimilar in color; when color was the same, form could not be matched. Each situation offered equal opportunity for matching upon the basis of either form or color, but a choice always had to be made.

Material. In the present study, three sets of material were used. The first set, which was used only with children below the age of six years, consisted of a series of solid geometrical forms painted in the six colors, *red*, *orange*, *yellow*, *green*, *blue* and *purple*. Oil paints which produced a surface having but slight gloss were used. The colors were bright and clear, but no attempt was made to match exact standards, since it was obvious that the soiling from constant handling by the children would quickly produce minor variations from the original colors in any case, and thus preclude precise control of the color factor. This difficulty was reduced to a minimum, however, by frequent cleaning with art-gum and by repainting the blocks at brief intervals throughout the experiment. The complete set included 6 two-inch wooden *spheres*, one in each of the six colors; 12 two-inch *cubes*, two in each of the six colors; and 18 *cylinders*, two inches in diameter and two inches in height, three in each of the six colors. In placing the forms before the children, the cube was used in two positions and the cylinder in three (see Fig. 1).

[1] The problem was suggested and the plan of procedure developed by Goodenough, who is also responsible for the method employed in summarizing the results and for writing the article. The data were collected by Brian.

[2] Alice Descoudres, Couleur, forme, ou nombre? *Arch. de psychol.*, 1914, *14*, 305–341.

The second set of material was also used only with the children under six. It consisted of six series of surface forms cut from heavy colored paper. The forms used were *circle*, *square*, *triangle*, a *diamond* formed by simply turning the square to the

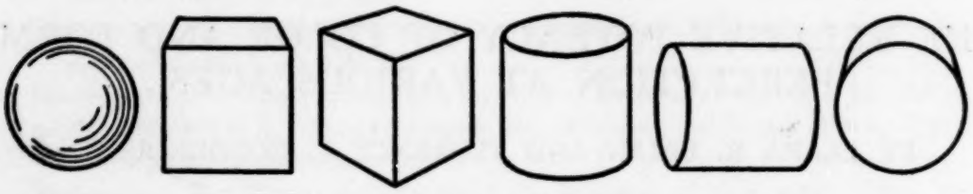

FIG. 1. Solid forms

diagonal position, a six-pointed *star*, and an *oblong* with rounded ends which was always placed with the straight edges parallel to the edge of the table (see Fig. 2). All forms were made in uniform two-inch dimensions. Each form was made in each of the six colors previously named.

FIG. 2. Surface forms

The third set of material was used only with the children above six years of age and with the adult group. It differed from the series last described only in that the forms were pasted in position on sheets of white paper, one sheet for each subject, thus making it possible to secure the data by means of a group experiment. The same colors and forms were used as in the series just described.

Subjects. The total number of subjects used in the experiment was 474, divided into four groups as follows.

1. Children enrolled in the nursery schools at the University of Minnesota Institute of Child Welfare and the Iowa Child Welfare Research Station. A total of 76 cases, 41 boys and 35 girls, ranging in age from 22 months to 5 years, 3 months was included in this group. The data were obtained during the spring and summer of 1927. Each child was seen individually, but only the solid forms were used.

2. A group of 143 cases, 78 boys and 65 girls, ranging in age from 14 months to 5 years, 11 months. These children were obtained from the nursery school of the University of Minnesota Institute of Child Welfare and from several Minneapolis day nurseries. The data were obtained during the spring of 1928. The group includes 20 children who had been used as subjects the previous year; but since a full year had elapsed between the two series of experiments, it seemed justifiable to include them. Individual experimentation was again used, and, with a few exceptions, both the solid and the surface forms were tried.

3. A total of 216 elementary school children, ranging in age from 6 to 14 years, served as subjects in a group experiment, using the third set of material described previously. This set, it will be recalled, included only the surface forms.

4. A group of 40 adult women enrolled in a study group for parental education were also used as subjects. The procedure was the same as that used for the grade school children.

Experimental procedure. In the case of the pre-school children who served as subjects in the individual experiments, the procedure was as follows. The child was invited to come to the laboratory to 'see some new blocks.' There he was seated at a low table facing the experimenter, who said, "Now watch, and see what I have to show you." A screen was then interposed and the experimenter removed two of the forms, differing both in form and color, from a covered box at her right and arranged them behind the screen, placing them 8 inches apart and equidistant from the child. The screen was then removed and a third block, chosen so as to match one of the first two blocks in respect to form and the other in respect to color was placed in a position intermediate between the first two blocks and 6 inches nearer to the child (see Fig. 3*a*).

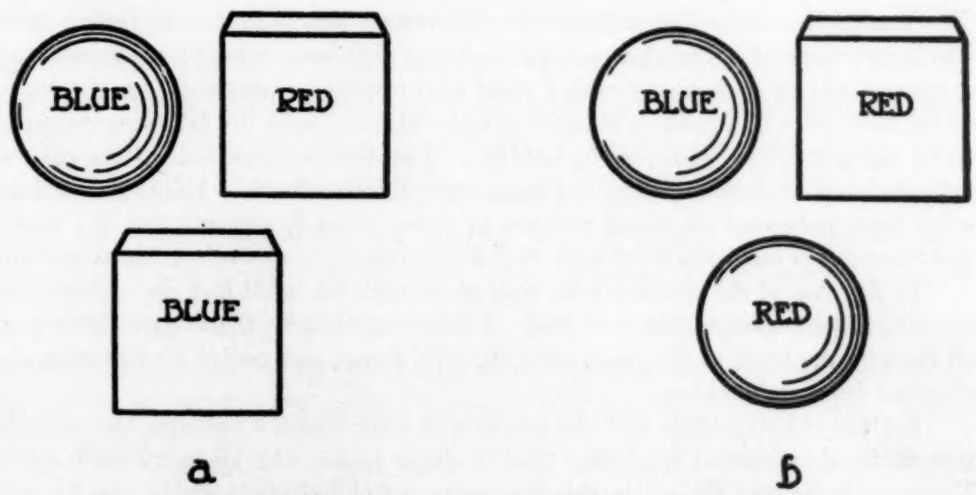

FIG. 3. Illustrating placement of forms before child

Constancy of distance and position was insured by using as a base a sheet of neutral gray cardboard with crosses to indicate the points at which the blocks were to be placed. The experimenter then said, "See these two pretty blocks? And see this other one. Now which of the two blocks up here is just like this one?" (pointing to the single block). The question was repeated if the child did not appear to comprehend.

In the outset of the experiment the question used was, "Show me the one that is nearest like this one." This method was discontinued in favor of the more dogmatic question after the first few experiments, since it was found that the younger children had difficulty in understanding it, and since, for the greater part, these young children showed no evidence of observing more than a single type of resemblance. Their choices were usually made without hesitation, and were frequently accompanied by such positive expressions as "Here it is," "This one's like that," or, in some instances,

the basis for the choice was stated specifically as, "Red and red, these are alike," "Two round ones," or "See, they stack," piling two cubes one upon another. Of the entire group of 218 pre-school children, only 3 gave verbal evidence of having observed any lack of absolute similarity between the two blocks chosen. One five-year-old boy hesitated for an instant, then matched a yellow cylinder with an orange cylinder, remarking as he did so, "These" (indicating the yellow cylinder and the yellow cube) "are colored alike, but these can roll." Another child matched a blue with a red star at once, but commented, "This *looks* like that" (pointing to the blue triangle) "but they are supposed to stack." A third remarked, "This is a good star," but nevertheless matched on the basis of color. None of the remaining cases appeared to notice any other attribute of the figure than that which formed the basis for their matching.

As soon as the choice had been made, the screen was replaced and the third block removed. The first two blocks were left in their original position while the third block was exchanged for another in which the color and form combination was reversed from that used originally (see Fig. 3*b*). A child who had first chosen the block on the left would then have to choose the one on the right or else reverse the apparent basis for his choice.

The screen was then replaced and an entirely new series of blocks of different form and color were presented, and both alternatives presented successively as before. This was continued until six different combinations of blocks, involving twelve choices, had been presented. The right and left positions of the two upper blocks were changed systematically in such a way that a child who matched consistently on the basis of either form or color would be obliged to make his choices in the following order; left, right, right, left, left, right, right, left, etc. The different positions of the cube and cylinder were treated as if they had been truly different forms. Each form and each color were presented an equal number of times to each subject, and the possible combinations of form and color were varied systematically throughout the experiment.

In the case of the children who were given both the solid and the surface forms, the former were always presented first. The procedure with the surface forms was in all respects the same as that used with the solid forms, and twelve choices were again obtained from each subject.

For the older children and the adults who were tested in groups, the procedure was modified in several respects. Only a single choice was made by each subject. The experiment was limited in this way since individual trials with a small number of older subjects had shown the difficulty involved in getting these cases to react repeatedly on a simple perceptive level. After one or two choices had been made, the entire procedure appeared to have become highly verbalized, and as the nature of the problem, which was usually realized in the outset by these more mature individuals, became more and more clearly formulated in their minds, intellection rather than perception appeared to become more and more the dominant element in determining the choice. This was indicated by the nature of their comments. With some, the later choices were made in a perfunctory manner after a brief glance at the objects, together with such remarks as, "Of course, it's what the thing *is* that matters, not what color it happens to be." Others became increasingly introspective and confused. "It's always form or color, but I can't decide which I ought to match." In both cases, the attempt to make the decision upon the basis of a general guiding principle rather than in terms of the immediate situation was clearly evident. It must be remembered that none of these subjects had any acquaintance with psychological experimentation. With trained subjects, this difficulty could probably have been overcome.

The procedure finally adopted was as follows. Each subject was provided with a sheet of paper upon which a single series of forms had been pasted in position. The dimensions of the forms and the distances at which they were placed were identical with those used for the individual experiments. The sheets were placed before the subjects face downward, and they were told not to turn them over until the signal was given. Names and dates of birth were then written upon the backs of the sheets, the teachers assisting in the case of the younger children. Instructions were then given as follows:

"On the other side of these sheets, you will find that some papers have been pasted. There are two papers at the top of the sheet and one at the bottom. When I tell you to turn the sheets over, you are to look quickly at the bottom paper and then at the two papers at the top, and draw a line from the one at the bottom to whichever one of the two top papers is most nearly like it. Neither of them will be exactly the same, but that doesn't matter. Draw the line from the bottom paper to the top paper which looks most like it. Do it as quickly as you can. Ready, go!"

Speed was emphasized in order to reduce to a minimum the rationalization or the formulation of a rule. Papers were collected at once so that no subject should have an opportunity to modify his original choice.

Results. In the case of the very little children it was quite evident that the verbal instructions had been very imperfectly comprehended. In spite of this, the results obtained were sufficiently consistent to demonstrate rather clearly that some factor other than chance had been operative in determining the matching in many instances. The general trend of the reactions at these early ages was, however, obscured to some extent by the strong tendency toward perseveration of response which has in general been found characteristic of very young children, and which was displayed to a rather marked extent by our subjects who were under three years of age, and to a somewhat less degree by the older children of the pre-school group. A child who had chosen a certain block or figure on the first trial in a given series was very likely to choose the same one on the second trial, thus apparently reversing the basis for his choice. While it is quite possible that a small percentage of these cases represented a real attempt at matching and a true reversal of opinion, it seems far more probable that in most cases the choice was made upon the basis of individual interest or pure chance, rather than any observed similarity between the objects which were chosen.

In treating the data it seemed better, therefore, to disregard all perseverative responses and to consider only the

cases in which, either by accident or design, both choices for a given set of material were made on a uniform basis. Where a perseverative response was given, the first as well as the second choice was thrown out.

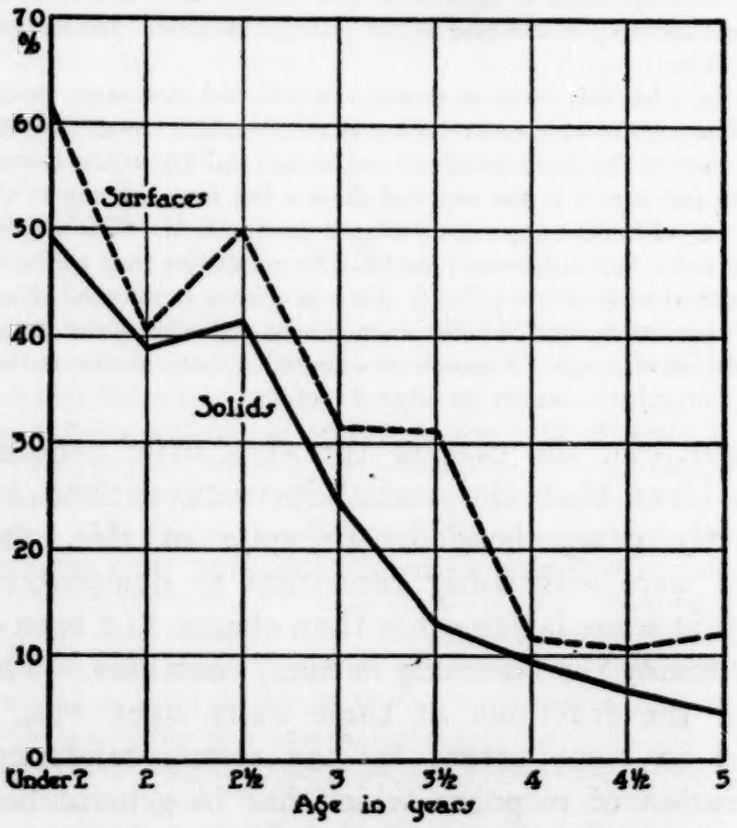

FIG. 4. Percentage of perseverative choices by age

Table I shows the decrease with age in the proportionate number of perseverative choices for the solid and the surface forms separately. These results are shown graphically in Fig. 4.

TABLE I

PERCENTAGES OF PERSEVERATIVE CHOICES BY AGE
(Based on second choices only)

	Under 2	2–0 2–5	2–6 2–11	3–0 3–5	3–6 3–11	4–0 4–5	4–6 4–11	5–0 5–11
Solids	49.1	38.9	41.6	24.5	13.2	9.3	6.4	4.4
Surfaces	61.5	40.3	50.0	31.3	31.2	11.8	10.7	12.1
Total	54.9	39.4	45.2	27.2	20.7	10.1	8.2	7.7

It is seen that while the children under two years of age show the perseverative tendency in more than half of their

total number of responses, this tendency falls off steadily with increasing age, until by the age of five years it is apparent in less than 8 per cent of the responses. At every age level, the surface forms show a greater proportion of responses of the perseverative type than do the solid forms.

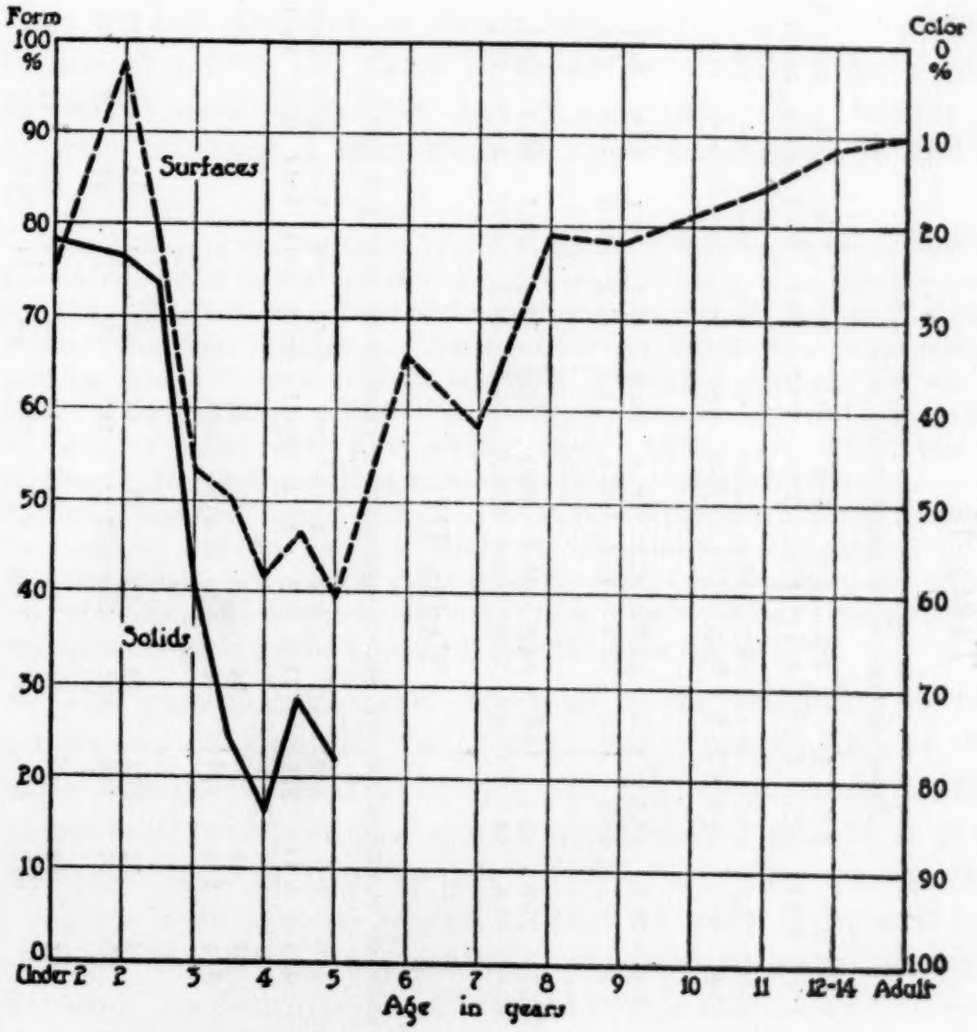

FIG. 5. Percentages of form and color choices by age (based on non-perseverative choices only)

Fig. 5 and Table II show, for the solid and the surface forms separately, the percentage of the total number of non-perseverative choices which were made on the basis of form and color respectively. The results from the older children and the adult group are also included. It will be recalled that only the surface figures were used with the older sub-

TABLE II

PERCENTAGES OF FORM AND COLOR CHOICES BY AGE
(Based on non-perseverative choices only)

	Under 2	2–0 2–5	2–6 2–11	3–0 3–5	3–6 3–11	4–0 4–5	4–6 4–11	5–0 5–11	6–0 6–11	7–0 7–11	8–0 8–11	9–0 9–11	10–0 10–11	11–0 11–11	12–0 13–11	Adult
Solids																
Form	78.2	76.6	73.7	40.5	24.2	16.7	28.7	22.1	—	—	—	—	—	—	—	—
Color	21.8	23.4	26.3	59.5	75.8	83.3	71.3	77.9	—	—	—	—	—	—	—	—
Surfaces																
Form	75.7	97.7	79.5	53.5	50.6	41.8	46.7	39.6	65.9	58.3	79.1	78.3	81.5	84.6	88.8	90.0
Color	24.3	2.3	20.5	46.5	49.4	58.2	53.3	60.4	34.1	41.7	20.9	21.7	18.5	15.4	11.2	10.0
Total																
Form	77.2	84.2	77.6	45.4	33.6	24.7	36.0	29.3	65.9	58.3	79.1	78.3	81.5	84.6	88.8	90.0
Color	22.8	15.8	22.4	54.6	66.4	75.3	64.0	70.7	34.1	41.7	20.9	21.7	18.5	15.4	11.2	10.0

TABLE III

NUMBER OF SUBJECTS AND OF NON-PERSEVERATIVE CHOICES AT EACH AGE LEVEL

	Under 2	2–0 2–5	2–6 2–11	3–0 3–5	3–6 3–11	4–0 4–5	4–6 4–11	5–0 5–11	6–0 6–11	7–0 7–11	8–0 8–11	9–0 9–11	10–0 10–11	11–0 11–11	12–0 13–11	Adult
Solids																
Subjects	18	21	22	36	34	34	39	15								
N-P choices	110	154	152	326	354	370	438	172								
Surfaces																
Subjects	16	12	13	24	19	17	28	11	48	31	43	23	27	26	18	40
N-P choices	74	86	78	198	198	180	300	116	48	31	43	23	27	26	18	40
Total subjects	18	21	22	36	34	34	39	15	48	31	43	23	27	26	18	40
Total N-P choices	184	240	230	524	552	550	738	288	48	31	43	23	27	26	18	40

jects. The number of subjects and the total number of non-perseverative choices at each age-level upon which the percentages in this and the following tables have been based are shown in Table III.

It is evident that there is a certain period of life during which color appears to be accorded greater relative significance than at any previous or succeeding age-level. This period begins at about the third birthday or slightly before and continues until about the age of six years. The tendency appears to reach a maximum at approximately four and a half years.

Our findings in this respect are in general agreement with the results obtained by Descoudres, except for the fact that since her youngest group of subjects was composed of children between the ages of three and six years, the early preference for form was not brought out by her study. She states, however, that she tried the experiment with one child of twenty-six months, who made 22 out of 24 choices on the basis of form. In his *Psychology of Early Childhood* Stern quotes the results of a somewhat similar study carried out by Katz, who also found a decided preference for color as compared to form among children between the ages of 2 years, 9 months and 4 years, 8 months. Upon the basis of other observations, however, Stern is inclined to question the conclusion arrived at by Katz, namely, that the concept of color develops earlier than that of form. He refers in particular to the work of von Kuenburg with infants and very young children who found that in fitting boxes of different forms and colors together color was in general ignored and attention given only to form.[3]

The question of course arises as to the reliability of the differences obtained in the present study, particularly at the early age-levels where, in spite of the elimination of the perseverative responses, chance undoubtedly continues to play an appreciable part in the results. We have approached this problem from several angles. First, we have computed the ratio of the differences actually obtained at each age-level between the percentages of form as compared to color choices to the standard error of the differences, according to the usual method. The results are given in Table IV, which shows that the probability of securing by chance differences of the magnitude obtained between the proportion of form as compared to color choices is negligible. The figures for the cases below the age of six, however, have been computed upon the basis of the total number of non-perseverative

[3] We have been unable to find either of these articles. Stern gives a brief summary of the results but does not cite the exact reference in either case.

TABLE IV

RELIABILITY OF THE DIFFERENCES IN THE PERCENTAGES OF FORM VS. COLOR CHOICES AT DIFFERENT AGES
(Based on number of N-P choices at each age. Expressed in terms of Diff./Stand. Error of Difference)

	Under 2	2–0 2–5	2–6 2–11	3–0 3–5	3–6 3–11	4–0 4–5	4–6 4–11	5–0 5–11	6–0 6–11	7–0 7–11	8–0 8–11	9–0 9–11	10–0 10–11	11–0 11–11	12–0 13–11	Adult
Solids	10.0	9.4	9.5	4.9	16.1	24.1	13.9	12.5								
Surfaces	7.4	41.5	9.2	1.4	0.2	3.1	1.7	3.2	3.3	1.3	6.6	4.7	6.0	6.9	7.4	11.9
Total	12.3	20.7	14.3	3.0	11.6	18.4	11.2	10.9	3.3	1.3	6.6	4.7	6.0	6.9	7.4	11.9

choices made by all the subjects at each age-level, and not upon the basis of the number of subjects. They afford fairly convincing evidence that individuals of a given age who participated in this experiment were, upon the whole, guided by some common principle in making their choices; but they do not show to what extent we are warranted in assuming that the reactions of this group are typical of those of other individuals of corresponding age. In order to throw light on this question, we have retabulated the results in such a way as to show, for each age-group separately, the percentage of the total number of cases who made all their non-perseverative choices on one basis, and have then computed the standard error of the differences between the proportion who (in their non-perseverative choices) matched only on the basis of form as compared to those who matched only on the basis of color. We have also calculated the proportion of the cases at each age who made all twelve choices on a common basis. In this last computation, the early preference for form is obscured by reason of the large number of perseverative choices made by the youngest children. The results are shown in Tables V and VI and in Figs. 6 and 7.

TABLE V

CONSISTENCY OF FORM-COLOR CHOICES BY AGE
(Per cent of total cases)

	Under 2	2–0 2–5	2–6 2–11	3–0 3–5	3–6 3–11	4–0 4–5	4–6 4–11	5–0 5–11
Solids								
All N-P form...	55.6	47.6	40.9	33.3	11.8	5.9	28.2	13.3
All N-P color...	0.0	4.8	4.5	33.3	61.8	64.7	66.7	60.0
12 form.....	0.0	0.0	4.5	5.6	8.8	5.9	17.9	13.3
12 color.....	0.0	0.0	0.0	13.9	44.1	50.0	53.8	53.3
Surfaces								
All N-P form...	50.0	91.7	46.2	33.3	47.4	35.3	39.3	36.4
All N-P color...	0.0	0.0	7.7	20.8	42.1	58.8	39.3	45.5
12 form.....	0.0	0.0	0.0	8.3	31.6	29.4	21.4	18.2
12 color.....	0.0	0.0	0.0	8.3	26.3	35.3	32.1	54.5
Combined								
All N-P form...	52.9	63.6	42.9	33.3	24.5	15.7	32.8	23.1
All N-P color...	0.0	3.0	5.7	28.3	54.7	62.7	55.2	53.9
12 form.....	0.0	0.0	2.9	6.7	17.0	13.7	19.4	15.4
12 color.....	0.0	0.0	0.0	11.7	37.7	45.1	44.7	53.9

TABLE VI

RELIABILITY OF THE DIFFERENCES IN THE PERCENTAGE OF THE TOTAL NUMBER OF SUBJECTS AT EACH AGE WHOSE NON-PERSEVERATIVE CHOICES WERE EXCLUSIVELY FORM AS COMPARED TO THE CORRESPONDING PERCENTAGE WHOSE NON-PERSEVERATIVE CHOICES WERE EXCLUSIVELY COLOR

(Based on number of subjects at each age. Expressed in terms of Diff./Stand. Error of Difference)

	Under 2	2–0 2–5	2–6 2–11	3–0 3–5	3–6 3–11	4–0 4–5	4–6 4–11	5–0 5–11	6–0 6–11	7–0 7–11	8–0 8–11	9–0 9–11	10–0 10–11	11–0 11–11	12–0 13–11	Adult
Solids	3.4	3.6	3.2	0.0	5.0	6.4	3.7	3.0								
Surfaces	2.8	11.5	2.5	1.0	0.3	1.4	0.0	0.4	3.3	1.3	6.6	4.7	6.0	6.9	7.4	11.9

(All 12 choices on a common basis)

Solids	0	0	1.0	1.2	3.6	4.7	3.6	2.6
Surfaces	0	0	0	0	0.4	0.4	0.9	1.9

The table reads: Of the children under two years of age, 55.6 per cent of the subjects in the experiments using the solid forms matched only on the basis of form in their non-perseverative choices; *i.e.* whenever the perseverative tendency was broken away from, it was toward matching form. None of these subjects matched only on the basis of color, and none made all twelve choices on the basis of either

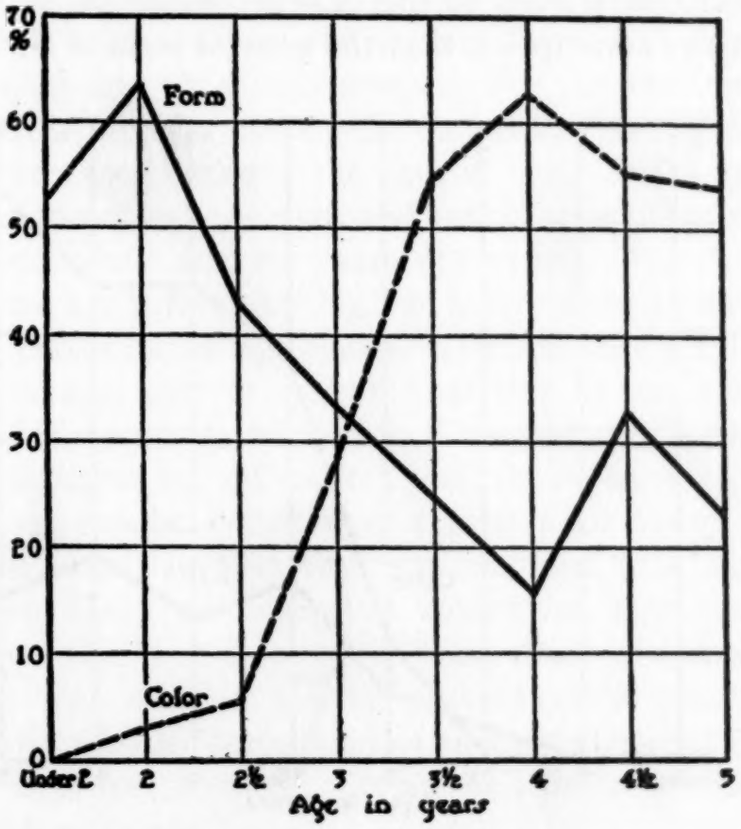

FIG. 6. Consistency of non-perseverative choices by age.
(Per cent of total cases who made all N-P choices on same basis.
Solids and surfaces combined.)

form or color, since at this age every child showed the perseverative tendency to some degree. Likewise, in the experiments using the surface forms, 50 per cent of the subjects matched form in all their non-perseverative choices, but none matched only on the basis of color. The percentages for the combined series have been computed on the basis of all experiments; that is, a subject who had both the solid and the surface forms has been counted twice.

Because of the reduction in the population when the number of subjects instead of the number of choices is made, the basis for the computation of the standard error of the differences in the percentages of form and color choices respectively, the ratios given in Table VI are considerably smaller than those shown in Table IV. Nevertheless they

are sufficiently large to establish, with few exceptions, the direction of the difference with a probability of error amounting to not more than one chance in several hundred. These facts, taken in conjunction with the comparatively even and regular characteristics of the changes which occur with age, warrant the assumption that the general form of the develop-

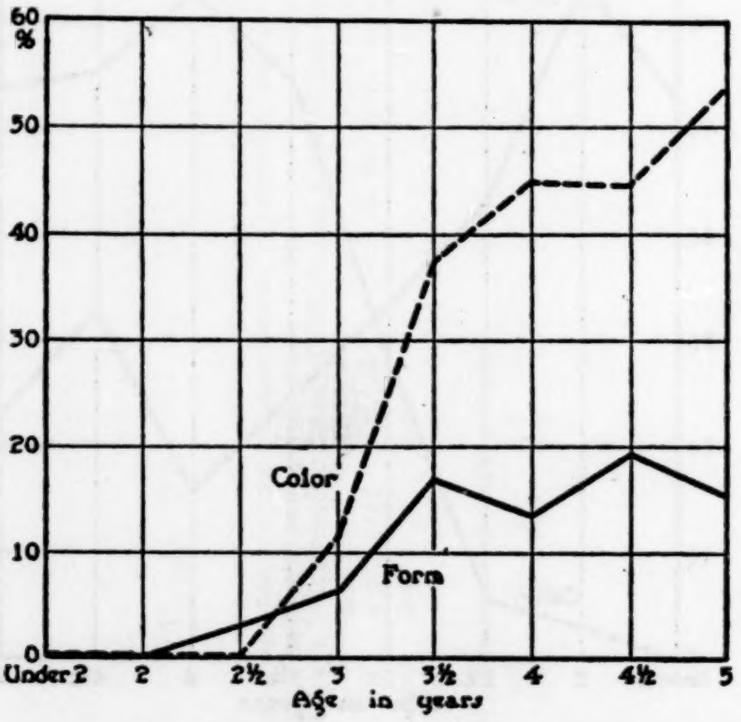

FIG. 7. Consistency of non-perseverative choices by age.
(Per cent of total cases who made all twelve choices on same basis.
Solids and surfaces combined.)

mental curve for the function under consideration has been established with reasonable accuracy. This assumption is further borne out by the correspondence between our results and those of other investigators.

The surface figures were apparently matched on the basis of form rather than color to a greater extent than the solid forms. The reason for this is not apparent. It may be due either to the particular figures used or to the fact that the solid forms were always given first in the series. It does not appear to be a result of the different sampling of cases, since the same general relationship is observed when only those

subjects who had both the solid and the surface forms are included. Within the ages observed, the general form of the curve is the same for both series.

Correlations (Pearsonian product-moment) were computed between the form-color ratios for the individual subjects on the solid as compared to the surface forms. The subjects were divided into three age-groups. For the children under three years of age (41 cases), the correlation was found to be + .545; for those between the ages of three and four-and-a-half years (60 cases), + .790; and for those between the ages of four-and-a-half and six years (39 cases), + .598. These correlations are sufficiently high to indicate the presence of a constant factor in the apprehension of objects by individuals of a given age, and to suggest that this factor is to some extent independent of the nature of the objects themselves.

No consistent sex differences were shown by our subjects. Mental test results, which were available for about half the pre-school cases, suggest that the children with high test-scores are likely to occupy a somewhat more advanced position on the form-color curve than those with low scores, and that they have a smaller percentage of perseverative choices and a larger percentage of entirely consistent choices. Further data are, however, needed in order to establish this point with certainty.

In planning the experiment, it was hoped that our facts would furnish some evidence as to the effect of a change in the position of a form (as in the case of the cylinders) upon the child's perception or recognition of the form. The results obtained do not warrant any statement at this time. A more intensive study of this factor is planned for the future.

Summary

1. In an attempt to ascertain whether form or color plays the greater part in the total apprehension of an object by individuals of a given age, an experimental situation was devised in which the subjects were required to choose between two alternatives in matching a series of geometrical solids and surfaces of different colors. Either form or color might be matched, but never both simultaneously.

2. A total of 474 subjects, ranging in age from children under two years to adults, were used. Exceedingly marked age differences in the relative potency of form and color as indicated by the matching were found; and statistical treatment showed that these differences are highly reliable.

3. Children below the age of three years exhibit a very marked tendency to match upon the basis of form rather than color. At about the age of three, a preference for color begins to manifest itself. Color is matched in preference to form by each half-year age-group between the ages of three and six years. By the age of six, form has again become the predominating factor in the situation, and the proportion of form as compared to color choices increases steadily from six years to the adult level. The 'peaks' in the curve occur at the ages of two to two-and-a-half, when 84 per cent of the choices are made upon the basis of form and only 16 per cent on the basis of color; at four to four-and-a-half, when 75 per cent of the choices are made on the basis of color and only 25 per cent on the basis of form; and at the adult level, when form accounts for 90 per cent of the choices and color for only 10 per cent.

4. The results of this experiment are in close agreement with those previously obtained by Descoudres when similar age-groups are compared. Since the Descoudres experiment included only one child below the age of three years, and grouped together all cases between the ages of three and six years, the form of the developmental curve for the early years was not clearly defined. So far as the writers are aware, the early predominance of form over color perception is here shown for the first time.

Note. Professor Bentley has suggested that the choices made by the younger children may not represent either their characteristic modes of perception or the general emphasis which they place upon the various attributes of perceived objects; but rather the particular connotation which, under adult tuition, such expressions as 'like' or 'unlike' have come to possess for them. The logical implications of these terms may not be the same for children as for adults.

The development of meaning is a fascinating problem for genetic research, since linguistic forms unquestionably take on changing connotations with age. Under ordinary conditions of modern life, it is probable that there is much uniformity in the

order in which the various 'meanings' which eventually come to be associated with any given verbal expression develop. In any social system, the development of language cannot be fortuitous. The fact that, in the present experiment, subjects of varying ages responded quite differently to the same verbal request in a given situation, and that the age-differences in the type of response corresponded very closely to those obtained by Descoudres with French children more than a decade ago, points to some relatively stable underlying factor. The nature of this factor can only be conjectured at present; but it seems improbable that direct adult training varies with the age of the child in such a way as to induce a preference for form at the age of two and a preference for color at the age of four. There are at least two alternative hypotheses. The first and simpler assumption is that the ability to discriminate colors is relatively late in developing, or that the reactions to color of young children, though present in some degree (as appears to be indicated by the work of Valentine, McDougall, Woolley and others), are nevertheless weak and poorly defined. Experimental evidence on this point is insufficient. It is also possible that we have here an illustration of the general process of concept development, or (as Hollingworth would put it) of the characteristic redintegrative sequence. When the child first begins to respond to partial elements in a situation he tends to select those which are common to many features of his environment. Objects are grouped by genus rather than by species. Thus all timepieces become 'clocks' or 'tick-tocks'; robins, crows, ducks, and hens are alike 'birds' or 'chickens,' and so on. In making these early classifications, structure and function are of major importance, while color can in most instances be ignored. A little later on, as facility in making gross classifications increases, the child becomes preoccupied with those attributes of a given object or situation which make for the differentiation of species within a given genus. Under these circumstances, color discrimination becomes more important. Still later, when a level of development has been reached at which the subject is able to shift his attention at will from one aspect of a situation to another, a new process of organization and evaluation of these partial elements takes place. The early interest in form might then be explained upon the basis of the major importance of form in the child's first attempts at organizing and classifying the objects of his environment; the later interest in color because of its importance in making finer discriminations between objects within classes whose broader features he has already mastered; while the final swing toward form is the result of the gradual organization of the various attributes of a given situation in terms of those factors which experience has shown are most frequently effective in determining appropriateness of response. Again, experimental evidence for the validation of the hypothesis is lacking.

(Manuscript received August 24, 1928)

FECHNER'S LAW AND THE METHOD OF EQUAL-APPEARING INTERVALS

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The purpose of the present experiment was partly to ascertain whether Fechner's law holds for a certain kind of stimulus but primarily to bring out certain limitations of the method of equal-appearing intervals and the ways in which some of them may be overcome.

One of the best known examples of the method of equal-appearing intervals in the experimental verification of Fechner's law is the Sanford weight experiment.[1] The envelopes are so weighted that they present as far as possible the same appearance and that they are in all respects identical except as to their weights. The subject is asked to sort out these envelopes into five piles so that the piles seem to be equally spaced as to apparent weight. Each pile of envelopes is weighed and then it is easy to ascertain the average weight of the envelopes in each of the five piles. The five equally-spaced *S*-values, the five successive piles, are then plotted against the average weight of the envelopes in each of the five piles. The plot is expected to be logarithmic to satisfy Fechner's law. This procedure involves a methodological error which can be avoided by the procedure to be explained.

The stimuli for the present experiment consisted of a series of 96 cards. On each card was printed a $2\frac{1}{2}$-inch square which was filled with irregularly spaced dots. The stimulus magnitude was the number of dots in the square. The lowest and the highest magnitudes are illustrated in Fig. 1. They were arranged in a geometrical series of 24 steps from 68 dots to 198 dots. Each stimulus magnitude was represented by four cards in the series. The subject was given

[1] E. B. Titchener, *Experimental Psychology*, vol. 2, pt. i, 1905, 33; E. C. Sanford, *A course in experimental psychology*, pt. i, 1898, 340.

the pile of 96 cards and asked to sort them out into ten piles which should constitute as far as possible a series evenly graduated as to the apparent density of the dots on the cards. There were 101 subjects in the experiment so that there were 404 sortings for each stimulus magnitude.

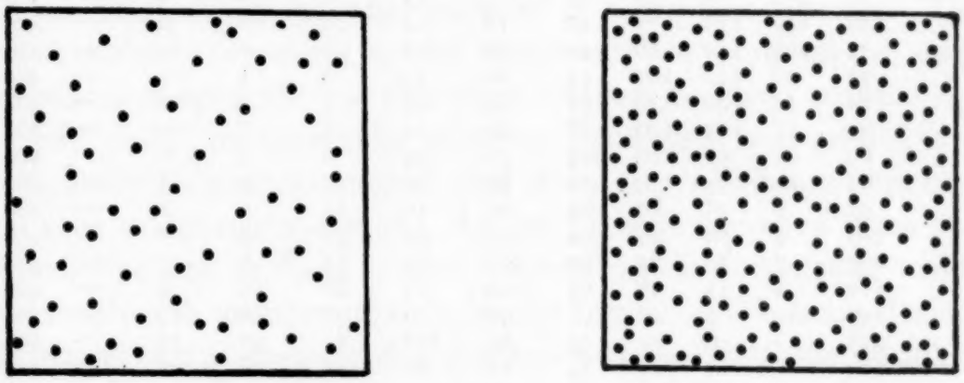

Fig. 1

The raw data are summarized in Table I. The *S*-scale is represented by ten equal class intervals, the ten piles. The *R*-scale is represented by the number of dots on the card.

Fechner's law is a statement of the relation between these two variables. It is usually written in the form

$$S = K \log R, \tag{1}$$

in which S is the allocation of the stimulus to the psychological or discriminal continuum, and R is the stimulus magnitude, the number of dots. In order to provide for any units of measurement for R and S, and in order to provide for an arbitrary origin on the S-scale it is desirable to write the equation in the more flexible form

$$S = K \log (CR). \tag{2}$$

This equation is easily rectified by writing it in the form

$$S = K \log R + K \log C, \tag{3}$$

in which $K \log C$ is a constant. The verification of Fechner's law would be complete if the plot of S against $\log R$ should be linear. If this plot is not linear, then Fechner's law is not verified. This assumes, of course, as in any experimentation

TABLE I

The S-scale in ten equal-appearing intervals (piles)

No. dots	0–1	1–2	2–3	3–4	4–5	5–6	6–7	7–8	8–9	9–10	Total no. sortings
68	246	85	38	27	4	3	—	—	1	—	404
71	147	123	77	40	12	2	1	—	2	—	404
74	169	112	66	42	12	2	1	—	—	—	404
77	72	140	103	53	20	12	3	1	—	—	404
81	51	122	126	51	34	13	5	1	1	—	404
84	55	113	120	58	40	12	3	3	—	—	404
89	13	59	126	104	56	34	8	4	—	—	404
93	14	54	106	92	69	39	20	7	3	—	404
97	8	53	89	104	95	40	11	2	2	—	404
102	1	30	59	102	119	52	32	9	—	—	404
107	7	26	73	92	115	49	34	8	—	—	404
112	1	21	26	73	119	75	68	16	5	—	404
117	—	9	26	57	90	79	97	36	10	—	404
123	—	6	11	34	84	118	89	47	15	—	404
129	—	3	15	34	71	88	113	58	21	1	404
135	—	4	14	39	59	107	101	63	17	—	404
142	—	2	4	15	28	73	122	92	64	4	404
149	—	—	—	6	25	58	112	122	69	12	404
156	—	—	5	8	15	36	89	117	104	30	404
164	—	—	1	3	14	27	61	127	123	48	404
172	—	—	1	6	13	21	43	108	143	69	404
180	—	—	1	2	3	14	23	76	154	131	404
189	—	—	—	2	1	9	14	40	116	222	404
198	—	—	—	—	1	1	6	16	83	297	404
	784	962	1087	1044	1099	964	1056	953	933	814	(9696)
Average no. dots in each pile..	74.19	82.71	89.75	99.84	110.59	124.06	135.39	151.59	166.97	185.81	

of this type, that there is sufficient curvature to the plot S against R so that its rectification by the above logarithmic plot is convincing.

The detailed interpretation of the table is as follows. There were 246 sortings in which cards containing 68 dots were placed in the first or lowest of the ten piles. The cards containing 68 dots were placed in the second of the ten piles 85 times. It is to be expected that the cards with relatively low stimulus magnitudes (number of dots) should be placed in the lowest piles while the cards with the large number of dots should be placed in the higher numbered piles. This is seen to be the case in that there is a drift of entries in the table from the upper left-hand corner to the lower right-hand corner. Since each stimulus magnitude was sorted 404

times, the sum of the entries in each row is 404. The piles were designated as equal class intervals on the *S*-scale and the origin for the scale is placed arbitrarily at the lower edge of the lowest of these ten class intervals. For this reason the entries in the first of the ten columns can be handled as class frequencies for the interval 0–1 on the *S*-scale. The next column represents the frequency of sortings of each stimulus magnitude for the class interval 1–2 and so on.

In order to ascertain whether Fechner's law holds, it is necessary to compare the *S*- and *R*-values, which are expected to give a logarithmic plot. In the present instance there can hardly be any ambiguity about the stimulus magnitude which is simply the number of dots in the square. We must decide, however, on a method for determining the *S*-value which is to be assigned to each stimulus magnitude. At this point appears the methodological error which is involved in the well-known Sanford weight experiment, which has been quoted by Titchener and others. If we should proceed as in the Sanford weight experiment, we should merely ascertain the average number of dots in each pile. These would be considered as the adjusted *R*-values to be plotted against the equal intervals on the *S*-scale. As a matter of fact such a procedure makes use of the wrong regression for this problem.

In many experimental problems it is necessary to ascertain which of two regressions is to be used. Whenever the object is prediction one should use the regression "unknown on known." In the present case we are not interested in predicting *S*-values from known *R*-values, or *vice versa*. We are interested in the intrinsic relation between *S* and *R*. In such a situation one should minimize experimental discrepancies in the variable which is least accurately measured. In this case there can be no doubt that the *S*-value is far more inaccurate than the *R*-value, which is merely the number of dots on the card. Consequently we should adjust the *S*-values against constant *R*-values. The regression "*S* on *R*" assumes that the *R*-values contain no error and that is for all practical purposes true. Probably the only error in the present *R*-values is the very slight fluctuation in the diameters

of the dots by which apparent density might conceivably be affected. But this error is almost infinitesimal compared with the discrepancies in the S-values, the apparent magnitude. In a situation in which both variables are measured with equal accuracy, and in which no problem of prediction is involved, one should fit the data by a function intermediate between the two regressions. For linear functions this is easily done, but non-linear functions for such experiments are sometimes troublesome.

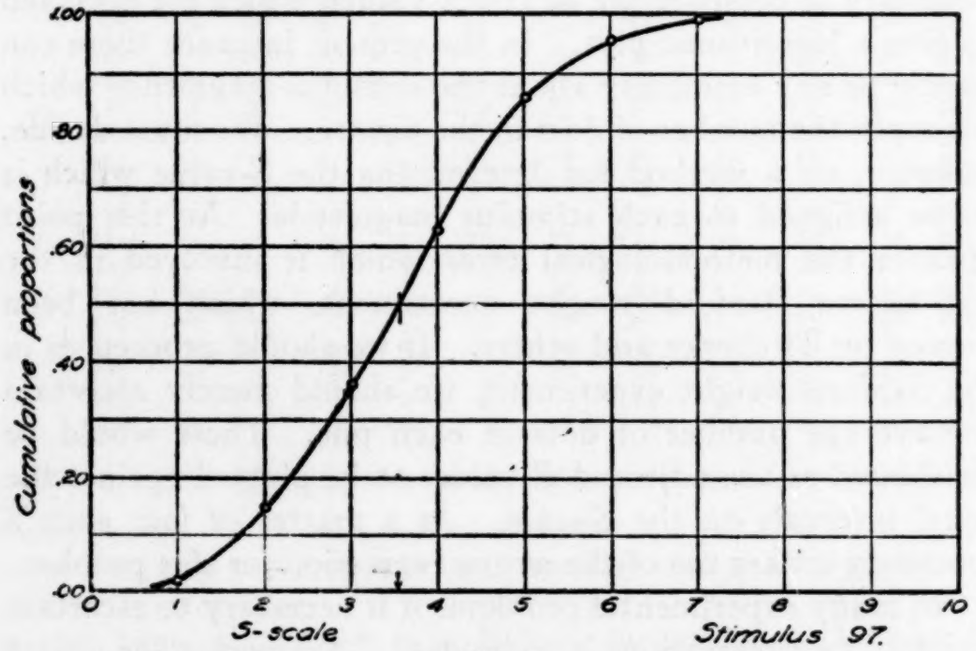

FIG. 2

The instructions for the Sanford weight experiment call for the regression R on S, which is a violation of good experimental practice. We shall use the correct regression in this case which is S on R. This necessitates that we ascertain an S-value for each of the 24 stimulus magnitudes. Inspection of our table shows immediately that the distributions represented by the horizontal rows are by no means normal. They are banked toward the end piles by the "end-effect." Merely to ascertain the arithmetic mean of the piles is therefore not a suitable procedure. This objection also holds against the incorrect procedure of the Sanford weight experiment in which the student is asked merely to ascertain the

average weight per envelope in each pile. We have adopted the median *S*-value for each stimulus magnitude as the most suitable measure of central tendency on the *S*-scale. For this purpose a cumulative frequency distribution was drawn for each of the horizontal rows of the table. An example of this procedure is illustrated in Fig. 2 for stimulus magnitude 97. The class frequencies for the *R*-value of 97 have here been represented in a cumulative frequency distribution of the percentile form. The median *S*-value is the *S*-value at which the smoothed curve reaches an elevation of .50, which is 3.55 for the stimulus 97. This means merely that half of the subjects perceive this stimulus as larger than 3.55 on the *S*-scale, while half of them perceive it as smaller than that apparent magnitude. One cumulative frequency diagram was drawn for each stimulus so that there were 24 diagrams for this experiment similar to Fig. 2.

TABLE II

Number of dots R	S_e	log R	$k \cdot \log R$	Median *S*-value S_e
68	0.65	1.8325	34.1248	0.7113
71	1.40	1.8513	34.4749	1.0614
74	1.30	1.8692	34.8082	1.3947
77	1.90	1.8865	35.1304	1.7169
81	2.20	1.9085	35.5401	2.1266
84	2.30	1.9243	35.8343	2.4208
89	3.05	1.9494	36.3017	2.8882
93	3.30	1.9685	36.6574	3.2439
97	3.55	1.9868	36.9982	3.5847
102	4.10	2.0086	37.4041	3.9906
107	4.05	2.0294	37.7915	4.3780
112	4.70	2.0492	38.1602	4.7467
117	5.20	2.0682	38.5140	5.1005
123	5.50	2.0899	38.9181	5.5046
129	5.85	2.1106	39.3036	5.8901
135	5.85	2.1303	39.6704	6.2569
142	6.65	2.1523	40.0801	6.6666
149	7.00	2.1732	40.4693	7.0558
156	7.45	2.1931	40.8399	7.4264
164	7.75	2.2148	41.2440	7.8305
172	8.10	2.2355	41.6295	8.2160
180	8.60	2.2553	41.9982	8.5847
189	9.10	2.2765	42.3930	8.9795
198	9.45	2.2967	42.7691	9.3556

We now have the experimental *S*-values, S_e. They are listed in Table II. We are now ready to determine whether

the linearity of Equation 3 is satisfied by the experimental data. The values of log R are also listed in the same table. In Fig. 3 we have plotted S against log R as indicated by Equation 3. The linearity of Fig. 3 is immediately apparent.

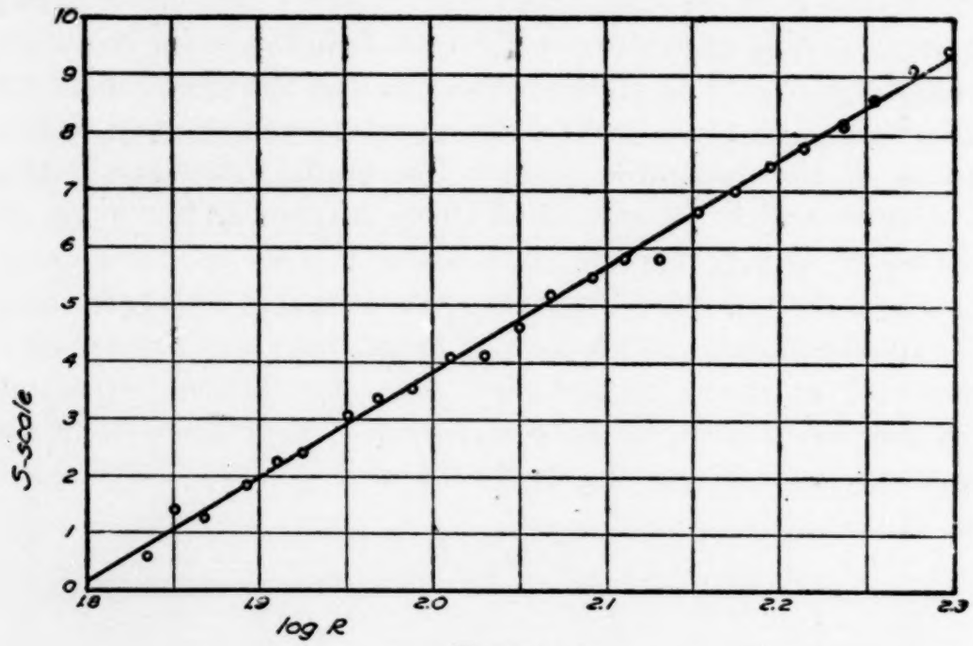

FIG. 3

Its slope is the constant K in Equation 3, and the S-intercept is the constant K log C. These constants may be determined by the method of least squares; but for the present problem that may hardly be necessary. The linearity of Fig. 3 constitutes final proof that Fechner's law holds for the stimuli of the present experiment. If Fig. 3 had been non-linear, it would have disproved Fechner's law for the present experiment. The straight line of this figure was drawn by inspection so that approximately as many points may be found on one side of the line as on the other. The constant K is determined graphically to be 18.621 and the S-intercept is also determined graphically to be -33.4135. These two values would undoubtedly shift slightly by different methods of fitting the line of Fig. 3 such as the method of least squares, the method of averages, or Reed's method of minimizing normals.[2] But for our purposes such slight fluctuations in the constants are not of importance.

[2] L. J. Reed, *Metron*, 1921, 1, 54–62.

The column $K \log R$ of Table II can then be filled in since the constant K has been determined. The last column of this table shows the calculated S-values, S_c. These are obtained again from Equation 3 with the known values for K, $K \log C$, and the known stimulus magnitudes.

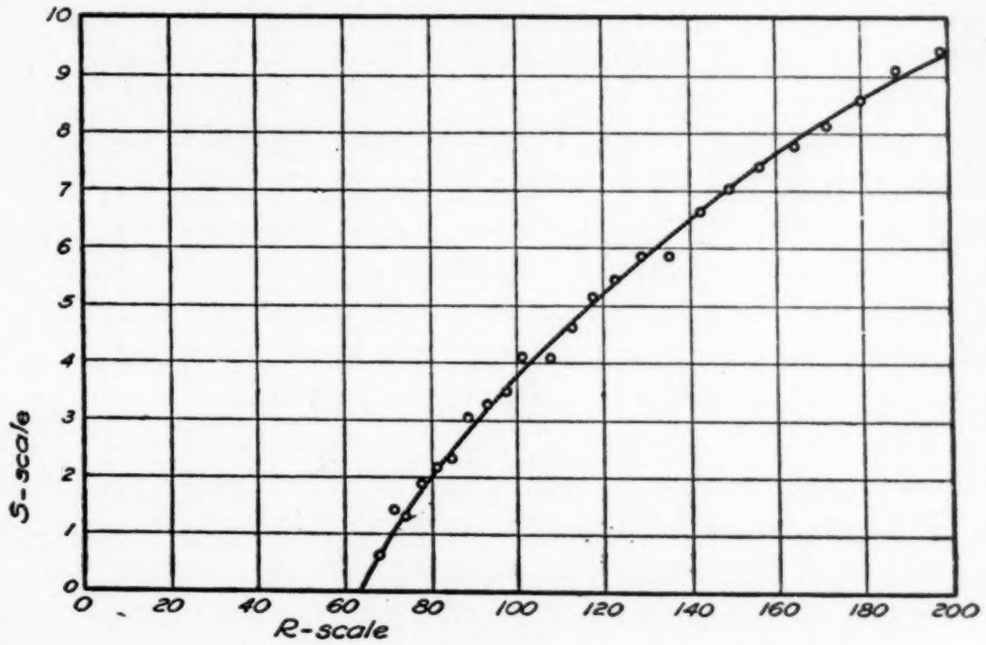

FIG. 4

In order to ascertain the magnitude of the experimental discrepancies from Fechner's law we have drawn Fig. 4. The small circles represent the experimental S-values, S_e, which have been plotted against the corresponding stimulus magnitudes. These data are obtained directly from Table II. The smooth curve has been drawn through the calculated S-values, S_c, of this table. This diagram shows rather clearly that experimental deviations from Fechner's law are very slight in the present experiment and we are therefore justified in concluding that the law has been verified.

There are several psychological and methodological considerations that are fairly well illustrated in this simple experiment. The final equation of the curve of Fig. 4 is

$$S = 18.621 \log R - 33.4135. \qquad (4)$$

One might inquire about the psychological meaning of this

equation when S is zero. In that case R takes the value 62. In other words, when there are about 62 dots on the card the S-value according to our equation should be zero. This is entirely sensible because the origin for the S-scale is arbitrary. In fact, according to Fechner's law, in its most general form, Equation 1 automatically prescribes an origin for the S-scale for unit stimulus magnitude. In the present case, 62 dots on the card happens to be unit stimulus magnitude for the law in its simplest form. There is no psychological ambiguity about this fact if we remember that we are dealing with an arbitrary origin on the S-scale. Naive interpretation might ask whether the apparent magnitude is zero for unit stimulus magnitude. But such a question is of course entirely irrelevant when S-measurements are made from an origin arbitrarily assigned to unit stimulus magnitude.

The same question may appear in slightly different form by inquiring about negative values for S. Myers [3] speaks of a "difficulty" about Fechner's law in that it leads to negative sensations. Any value for anything can be negative if you place the arbitrary origin high enough. And so in this case the S-value for a stimulus would be negative if the unit stimulus magnitude is arbitrarily assigned high enough. Consequently negative S-values in Fechner's equation do not mean that the apparent magnitude is less than nothing. This alleged difficulty with Fechner's law is caused by failure to understand the meaning of Fechner's simple equation. The issue simply does not exist.

It is of some interest to study the dispersion that a stimulus projects on the S-scale. This can be done by inspecting the data of Table I. The inspection shows the end effect rather conspicuously. This effect can probably be explained most simply in terms of the mechanical restrictions of the method of equal-appearing intervals. The subject adopts a scale of subjective magnitudes for the ten piles and when he sees an unusually large stimulus magnitude he may have the impulse to place it in pile 12 or pile 15 if such piles were available or allowable. Since he is restricted to ten

[3] C. S. Myers, *A textbook of experimental psychology*, 1909, 249.

piles, he sorts all of these large apparent magnitudes in pile 10. Undoubtedly the next stimulus which is slightly smaller is affected by such a restriction so that it is sorted into piles 8 or 9 instead of into 10 or 11. The same effect is seen at the lower end of the *S*-scale. For this reason we cannot deal with the distributions of Fig. 4 as in any sense true projections of the stimuli on the *S*-scale. They are subject to gross distortions by the mechanical limitations of the method of equal-appearing intervals. It is of course possible that we are also dealing with an adaptation effect caused by familiarity with the experimental stimulus range, and that this may itself constitute a source of distortion in addition to the mechanical limitation of the method of equal-appearing intervals. It is probably not so great as that of the restraint to a specified number of piles. There would seem to be good reason to assume that the distribution of the stimulus magnitude 198 should be approximately the same in form as the distribution for the stimulus magnitude 68. Consequently if we want to study the dispersion that a stimulus projects on the *S*-scale we must resort to other psychophysical methods than that of the method of equal-appearing intervals. In fact this is one of the most serious limitations of that method. It is entirely absent in the method of paired comparison, which is probably the best of all the psychophysical methods.

Another serious limitation of the method of equal-appearing intervals for which I can offer no solution is that the determinations are undoubtedly affected by the distribution of stimulus values which the experimenter happens to use. In the present case we use 24 stimulus magnitudes in a geometric series from 68 to 198 dots. If we had so selected the same number of stimuli as to constitute an arithmetical series with the same range, the subject would have been compelled to make the tenth pile rather large while the first pile would be rather small. It is almost certain that the subject would have been confused by the fact that his sortings gave gradually increasing piles from the first to the tenth. He might unwittingly disturb his scale of apparently equal intervals in an attempt to make the class frequencies in the

ten piles roughly equal. On the other hand, the same question may be raised as a criticism against our procedure, in which the stimulus magnitudes constituted a geometric series. If the subject sorts the stimuli into ten successive piles with instructions to make all of the ten piles approximately equal in size and without any instructions to make the intervals between them equal, then we should automatically verify Fechner's law, provided we were to interpret the intervals to be equal even though the subject were never told to make them so. This is a serious limitation in the method of equal-appearing intervals by which our confidence in this psychophysical method should be markedly disturbed. Whenever it is at all possible we certainly should avoid the method of equal-appearing intervals and use instead the method of paired comparison or one of its equivalents.

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A STUDY IN PHONETIC SYMBOLISM

BY EDWARD SAPIR

University of Chicago [1]

The symbolism of language is, or may be, twofold. By far the greater portion of its recognized content and structure is symbolic in a purely referential sense; in other words, the meaningful combinations of vowels and consonants (words, significant parts of words, and word groupings) derive their functional significance from the arbitrary associations between them and their meanings established by various societies in the course of an uncontrollably long period of historical development. That these associations are essentially arbitrary or conventional may be seen at once by considering such a proportion as

phonetic entity 'boy': idea (or reference) 'boy'
= phonetic entity 'man': idea (or reference) 'man.'

In passing from the notion of 'boy' to that of 'man' we experience a definite feeling of relationship between the two notions, that of increase in size and age. But the purely phonetic relationship of 'boy' : 'man' takes no account of this. So far as the referential symbolism of language is concerned, the words 'boy' and 'man' are discrete, incomparable phonetic entities, the sound-group b-o-y having no more to do with the sound-group m-a-n, in a possible scale of evaluated phonetic variants, than any randomly selected pair of sound-groups, say 'run' and 'bad,' have to do with each other.

This completely dissociated type of symbolism is of course familiar; it is of the very essence of linguistic form. But

[1] Publication of the Behavior Research Fund, the Institute for Juvenile Research, Chicago (Herman M. Adler, Director), Ser. B, No. 132. For valuable suggestions in the preparation of this paper I am indebted to Professor H. A. Carr, University of Chicago.

there are other types of linguistic expression that suggest a more fundamental, a psychologically primary, sort of symbolism.[2] As examples may be given the interrogative tone in such a spoken sentence as "You say he's dead?" in comparison with the simple declarative tone of the corresponding "You say he's dead"; further, the emphatically diminutive *ee* of *teeny* as contrasted with the normal *i* of *tiny*. In both of these examples the phonetic difference is undoubtedly felt as somehow directly expressive of the difference of meaning in a sense in which the contrast between say 'boy' and 'man' is not. We may call this type of symbolism 'expressive' as contrasted with the merely 'referential' symbolism which was first spoken of. It goes without saying that in actual speech referential and expressive symbolisms are pooled in a single expressive stream, the socialization of the tendency to expressive symbolism being far less extreme, in the great majority of languages, than of the tendency to fix references as such.

We may legitimately ask if there are, in the speech of a considerable percentage of normal individuals, certain preferential tendencies to expressive symbolism not only in the field of speech dynamics (stress, pitch, and varying quantities), but also in the field of phonetic material as ordinarily understood. Can it be shown, in other words, that symbolisms tend to work themselves out in vocalic and consonantal contrasts and scales in spite of the arbitrary allocations of these same vowels and consonants in the strictly socialized field of reference? The present paper is a preliminary report of certain aspects of a study, still in progress, intended to probe into any such latent symbolisms as may be thought to exist. The field of inquiry is vast and difficult to chart and I cannot hope to have guarded against all the possible fallacies of interpretation. For the present I have limited myself to the meaning contrast 'large' : 'small' as offering the most likely chance of arriving at relatively tangible results.

The main object of the study is to ascertain if there

[2] For the two symbolic layers in speech, as in all expression, see E. Sapir, Language as a form of human behavior, *Engl. J.*, 1927, 16, 421–433.

tends to be a feeling of the symbolic magnitude value of certain differences in vowels and consonants, regardless of the particular associations due to the presence of these vowels and consonants in meaningful words in the language of the speaker. The results so far obtained seem to go far in demonstrating the reality of such feelings, whatever may be their cause. It has also become very clear that individuals differ a good deal in the matter of sensitiveness to the symbolic suggestiveness of special sound contrasts.

A number of distinct schedules have been devised and applied in the research. In the early stages of the work the various types of sound difference were studied independently. For instance, the contrast between the vowel *a* and the vowel *i* (the phonetic or continental values are intended) was illustrated in every one of sixty pairs of stimulus words, the subject being requested to indicate in each case which of the two in themselves meaningless words meant the larger and which the smaller variety of an arbitrarily selected meaning. For example, the meaningless words *mal* and *mil* were pronounced in that order and given the arbitrary meaning 'table.' The subject decided whether *mal* seemed to symbolize a large or a small table as contrasted with the word *mil*.

In the first experiments schedules of sixty stimulus word-pairs were used, each of which was divided into two sections. The first thirty word-pairs involved only such sounds as the subject, an English-speaking person, would be familiar with, the second set of thirty word-pairs, while still illustrating the same phonetic contrast as the first thirty, say that of *a* to *i*, also involved sounds that the subject was not familiar with. Each of the two sets of thirty was further subdivided into functional groups: nouns, verbs with reference to large or small subject of verb, adjectives with reference to large or small things, verbs with reference to large or small object of verb, and verbs with reference to intense or normal degree of activity. It is important to note that the words were so selected as to avoid associations with meaningful words and it was the special purpose of the second set of thirty word-pairs to remove the subject still further from the intercurrent influence of meaningful linguistic associations.

If the results obtained from a considerable number of individuals can be relied upon as symptomatic, the influence of accidental, meaningful linguistic associations is less than might have been supposed, for the percentage of responses in favor of one of the two vowels as symbolizing the large object tended to be little less, if at all, in the second set of word-pairs than in the first. For example, Subj. IK found that of the first thirty word-pairs illustrating a contrast between the vowels *a* and *i* twenty-two examples of *a* "naturally" carried with them the connotation "large," five examples

of *i* carried this connotation, and three word-pairs were responded to indifferently. The effective score in favor of *a* as the vowel inherently symbolizing a large rather than a small reference was 22/27 or 81 per cent. In the second set of thirty word-pairs illustrating the same vocalic contrast, 21 of the words involving the vowel *a* were said to connote the large reference, 5 with the vowel *i* connoted the small reference, and 4 were indifferent. Here the effective score in favor of the symbolic value of the vowel *a* as large by contrast with *i* is 21/26 or, again, 81 per cent. In the case of the vowel contrast *a* to *e* (with the short value of the French *e*, as in *été*) IK's effective score in favor of the *a* vowel as connoting the larger reference was 24/29 or 83 per cent for the first 30 word-pairs, 73 per cent for the second 30 word-pairs.

The essential points that seemed to appear from these first experiments with individuals were; (1) that vocalic and consonantal contrasts tended with many, indeed with most, individuals to have a definite symbolic feeling-significance that seemed to have little relation to the associative values of actual words, (2) that it made surprisingly little difference whether the phonetic contrast was contained in a phonetically "possible" or a phonetically "impossible" context and (3) that the certainty of the symbolic distinction tended to vary with the nature of the phonetic contrast. The last point, which is important, will be discussed later on in this report.

These earlier experiments with individuals, though revealing, were felt as the work proceeded to be deficient in one important respect, namely, that the simple nature of the vocalic or consonantal contrast in a set of word-pairs might be expected to lead to a too ready systematization of responses on the part of the subject. In other words, the average subject could not help noticing after responding to a few stimuli that a certain consistency in the responses would naturally be expected, and that if the vowel *a*, for example, as contrasted with *e* or *i*, is felt satisfactorily to symbolize the larger of two objects, all other examples of word-pairs illustrating the same vocalic contrast should be dealt with in the same manner. The primary purpose of the experiment, however, was to elicit spontaneous feelings of symbolic contrast, unrevised by any judgment as to consistency of response. For this reason a further and, it is believed, much more efficient experiment was devised consisting of 100 word-pairs involving every type of phonetic contrast that was investigated. These hundred word-pairs were not arranged

in any logical order, nor was the order of the contrasted phonetic elements in any particular entry necessarily the same as in another entry involving the same contrast. In the table that was finally adopted the first word-pair illustrated the contrast between *a* and *i*, the second the contrast between *e* and *a*, the third the contrast between *z* and *s*, and so on through the list. The contrast between *a* and *i* was illustrated not only in Entry 1 but also in Entries 41, 81, and 87. In this way, it was hoped, systematization on the part of the subject was necessarily hindered, if not entirely blocked, and the responses actually obtained may be looked upon as normally spontaneous feeling judgments following in the wake of an initial suggestion as to preferred class of symbolic response (*i.e.* variations in magnitude).

For this second experiment 500 subjects were employed, most of them students of the University of Chicago High School. The subjects were eventually analyzed into the following groups; 6 cases of 11-year-old children, 30 of 12 years, 86 of 13 years, 94 of 14 years, 124 of 15 years, 81 of 16 years, 33 of 17 years, 10 of 18 years, 21 University of Chicago students, 8 adults who were not students and 7 Chinese. The subjects were provided with forms in which there were blank spaces for each of the entries, and they were carefully instructed to check off the first of the two stimulus words announced by the investigator as to whether it symbolized the larger or the smaller reference. If the response was indifferent, no check was to be entered in either the large or the small column. Very little difficulty was experienced in explaining the conditions of the experiment, which seemed to be enjoyed by the great majority of the subjects as a rather interesting game. It is believed that the results obtained are as reliable as material of this kind can be, every precaution having been taken to arrange conditions favoring simple and unambiguous responses and only the investigator himself pronouncing the stimulus words, in order that all confusion due to slight variations of pronunciation might be avoided.

The phonetic contrasts may be classified on phonetic and acoustic grounds into five main groups. There are also two minor groups which are of lesser interest. In the first group the contrasting vowels belong to the series *a*, *ä*, *ɛ*, *e*, *i*. The pronunciation of these vowels, as of all other vowels, was *quantitatively uniform in a given pair* in order that the independent symbolic suggestiveness of quantity differences as such be ruled out of consideration where quality alone was being studied. The phonetic values of these vowels were respectively those of *a* of German *Mann* (*a*), *a* of English *hat* (*ä*), *e* of English *met* (*ɛ*), *e* of French *été* (*e*), *i* of French

fini (*i*). It will be observed that the phonetic contrast is gradually lessened within the scale as one moves from *a* to *i*. Thus, *a* to *i* affords the greatest objective contrast, *ä* to *i* or *a* to *e* a lesser contrast, *ɛ* to *i* or *a* to *ɛ* a still lesser one, and *a* to *ä* or *ä* to *ɛ* or *ɛ* to *e* or *e* to *i* a minimal contrast. In other words, on purely objective phonetic grounds, one might imagine that the responses would tend to be further removed from a purely random or 50-50 distribution the greater the contrast between the vowels. It was therefore of great interest to determine not only whether there were preferred symbolisms, but also whether the varying percentages of response bore a fairly close relation to objective differences in the sounds themselves as determined on phonetic and acoustic grounds.

The second group of word-pairs illustrates the contrast between vowels on the scale *a*, *ɔ*, *o*, *u*, *i.e.* a scale with progressive lip-rounding. The third group illustrates contrasts between rounded back vowels (*u*, *o*, *ɔ*) and unrounded front vowels (*i*, *e*, *ɛ*, *ä*). In the fourth group of word-pairs there was illustrated the contrast between voiced and voiceless consonants, *e.g.* between *z* and *s*, *v* and *f*, *b* and *p*. The fifth group illustrates the contrast between stopped consonants and spirants or fricatives, *e.g.* between *f* and *p*, *x* (*ch* of German *Bach*) and *k*.

It would be quite impossible to report on all the details of the experiment in this place. I shall content myself with giving two selected tables. The first shows the distribution of responses for the word-pairs illustrating the contrast between *a* and *i*, classified according to the groups of subjects (11–18 yrs, university students, adults and Chinese).

Table I

Percentage of responses showing preference for *a* vs. *i* to symbolize 'large'

Entry no.	Obs. Age	6 11	30 12	86 13	94 14	124 15	81 16	33 17	10 18	21 Univ.	8 Adlts.	7 Chin.
1.....		83.3	86.7	90.6	92.3	83.1	84.0	78.8	80.0	85.0	100.0	100.0
41.....		100.0	70.0	82.7	78.0	76.4	71.6	69.7	50.0	95.2	100.0	85.7
81.....		83.3	93.3	74.7	72.2	81.8	80.0	77.4	100.0	70.0	85.7	85.7
87.....		83.3	83.3	84.1	86.0	91.8	86.1	72.7	80.0	90.0	100.0	42.9
Ave....		87.5	83.3	83.0	82.1	83.3	80.4	74.6½	77.5	85.0½	96.4	78.6

It will be observed that the percentage of responses in favor of *a* vs. *i* ranges all the way from about 75 per cent to about 96 per cent. For the largest group of subjects, the 124 fifteen-year-olds, the percentage is as high as 83, while the small number of 11-year-olds reach the figure 87.5. It is obvious that, regardless of infinite differences of an individual nature as to the general symbolic value of this phonetic contrast or as to its specific value in particular cases, English-speaking society does, for some reason or other, feel that of these two vowels, *a*, by and large, is possessed of a greater potential magnitude symbolism than the contrasted vowel *i*. The same feeling seems to be illustrated by the small number of Chinese cases. Furthermore, within the English-speaking community there seems little reason to believe that there is a significant growth in the firmness of the symbolic feeling after the age of 11. The case of the eight adults is not really significant because they consisted of high school teachers of English who answered the forms at the same time as their classes. They would naturally have a more self-conscious attitude toward the problem of sound symbolism than individuals selected at random. In other words, however these symbolisms are fixed, it is probable that they are so fixed at a rather early age and that familiarity with literature is not likely to count as a heavy factor in the situation. These general considerations are borne out by all the other findings, and it is of particular interest to note that the Chinese evidence is nearly always in the same general direction as that of the English-speaking subjects. Further work needs to be done on responses of this kind from younger children and from other groups of foreigners before the age and language factors can be properly evaluated or dismissed as irrelevant.

The second table is an attempt to show the differential symbolic value of the vocalic contrasts in the *a* to *i* series. Four age-groups (13–16), involving 385 subjects, are represented in this table. It was found in comparing the responses to the different vocalic pairs that they tended to arrange themselves roughly into four distinct groups (A, B, C, D).

TABLE II

CONFIGURATED DISTRIBUTION OF "*a* : *i*" RESPONSES IN AGES 13–16

	Age 13 (86 cases)	Age 14 (94 cases)	Age 15 (124 cases)	Age 16 (81 cases)
		a : *i* (4 steps) 82.1	*a* : *i* (4 steps) 83.3	
	a : *ɛ* (2 steps) 86.0			
Group A	*ä* : *i* (3 ") 84.7	*ä* : *i* (3 ") 80.3	*ä* : *i* (3 ") 80.0	*ä* : *i* (3 steps) 87.0
	a : *i* (4 ") 83.0			
	ɛ : *i* (2 ") 82.0			*ɛ* : *i* (2 ") 81.8
				a : *i* (4 ") 80.4
		ɛ : *i* (2 steps) 78.2	*ɛ* : *i* (2 steps) 76.8	
		a : *ɛ* (2 ") 76.9	*a* : *ɛ* (2 ") 72.8	*a* : *ɛ* (2 steps) 75.7
Group B	*ä* : *ɛ* (1 step) 76.4	*ä* : *ɛ* (1 ") 74.9		*ä* : *ɛ* (1 ") 74.8
	**a* : *e* (3 steps) 75.3	**a* : *e* (3 ") 73.1		
			e : *i* (1 ") 72.7	
			ä : *ɛ* (1 step) 69.5	
			**a* : *e* (3 steps) 68.6	**a* : *e* (3 steps) 70.7
Group C	*e* : *i* (1 step) 67.8	*e* : *i* (1 step) 67.5		*e* : *i* (1 ") 70.2
	a : *ä* (1 ") 62.5			
		ɛ : *e* (1 ") 60.3		
				ɛ : *e* (1 step) 60.4
Group D		*a* : *ä* (1 step) 56.5	*a* : *ä* (1 step) 59.0	*a* : *ä* (1 ") 58.7
	ɛ : *e* (1 step) 53.6		*ɛ* : *e* (1 ") 58.3	

In the first group, typically illustrated by the contrast between *a* and *i* and *ä* and *i*, the percentage of a response in favor of the vowel nearer *a* of the scale ranged from 80 per cent upward. The second group of responses was found to be somewhat set off from the preceding one by a marked decrease in the percentage of responses favoring the vowel toward *a* of the scale. This group is typically illustrated by the contrast between *a* and *ɛ*, the percentage in favor of the 'larger' vowel running from about 73 per cent to 78 per cent. The third group, illustrated by the typical contrast *e* to *i*, ranges from about 60 per cent to 70 per cent. The last group, that of minimal psychological contrast in the *a* to *i* set, runs below 60 per cent in favor of the vowel toward *a* of the scale.

The table has been arranged chiefly from the point of view of the internal 'hiatus' between the percentages of response within each age-group. It is noteworthy that the 'configurated distribution' of the responses runs fairly parallel in the four age groups both as to the stepwise discriminations which seem to be felt by many of the subjects and as to the actual order of the specific vocalic contrasts when evaluated by means of percentages in favor of the vowel toward *a* of the scale. Naturally, the reality and normal limits of these stepwise discriminations need to be tested by a careful examination of the individual records, supplemented by further experiments.

On the whole, it will be observed that the symbolic discriminations run encouragingly parallel to the objective ones based on phonetic considerations. This may mean that the chances of the responses being to a high degree determined by actual word associations of the language of the subject are slim, the meanings of words not being distributed, so far as known, according to any principle of sound values as such; and, further, that we are really dealing with a measurably independent psychological factor that for want of a better term may be called 'phonetic symbolism.'

One vocalic contrast, however, falls out of the expected picture. This is the *a* to *e* set, which is starred in the table.

Though the *a* vowel is judged prevailingly 'large' as contrasted with *e*, there seems to be present some factor of hesitation which lessens the value of the contrast. If we go by objective distances between vowels, the *a* to *e* contrast, being a '3-step' one, should have fallen into Group A, instead of which it actually either comes last in Group B or falls even as low as Group C. I believe that a very interesting and sufficient reason can be given for this curious fact. The short vowel *e*, as in French *été*, is not native to the English language. Subjects hearing the vowel *e*, when pronounced in the proximity of *a*, which is acoustically far removed from it, would tend not to hear what was actually pronounced, but to project the characteristic long '*e*-vowel' familiar to us in such words as *raise* or *lake*. In other words, the qualitative symbolism would tend to receive a revision in the opposite sense because of an intercurrent quantitative symbolism. This example is suggestive as illustrating the importance of the linguistic factor vs. the merely phonetic one, though not in the sense in which the term 'linguistic factor' is ordinarily understood. What skews the picture here is probably not the associative power of particular English words but the *phonetic configuration* of English as such.[3] That even this configuration, however, is of limited importance in interpreting the experiment is shown by the fact that in word-pairs illustrating the contrast *e* to *i*, ɛ to *e*, the acoustic nearness of the two vowels prevents the unconsciously imputed quantitative interference from making itself felt in the symbolic response.

These and many other similar results need interpretation. One's first temptation is to look about for some peculiarity of English speech, some distribution of sounds in actual words, that would make the results we have secured intelligible. A simple associational explanation, however, is not likely to prove tenable. The weighting of the responses is altogether too much in accordance with an absolute phonetic

[3] For the significance in language of 'sound patterns' or 'phonetic configurations' as distinct from sounds as such, see E. Sapir, Sound patterns in language, *Language*, 1925, 1, 37–51.

scale to make it possible in the long run to avoid at least some use of 'natural' or 'expressive,' as contrasted with socially fixed verbal, symbolism as an explanation. It is difficult to resist the conclusion that in some way a significant proportion of normal people feel that, other things being equal, a word with the vowel *a* is likely to symbolize something larger than a similar word with the vowel *i*, or *e*, or *ɛ*, or *ä*. To put it roughly, certain vowels and certain consonants 'sound bigger' than others. It would be an important check to amass a large number of randomly distributed meaningful words, to classify into the two groups of 'large' and 'small' those which could be so classified without serious difficulty, and to see if in sets in which equal numbers of phonetically contrasted words are found the meaning classes were or were not correlated with the sound classes and to see further, if they are so correlated, if the distributions are of the same nature as those studied in the experiments.

The reason for this unconscious symbolism, the factor of linguistic interference being set aside for the present, may be acoustic or kinesthetic or a combination of both. It is possible that the inherent 'volume' of certain vowels is greater than that of others and that this factor alone is sufficient to explain the results of the experiment. On the other hand, it should be noted that one may unconsciously feel that the tongue position for one vowel is symbolically 'large' as contrasted with the tongue position for another. In the case of *i* the tongue is high up toward the roof of the mouth and articulates pretty well forward. In other words, the vibrating column of air is passing through a narrow resonance chamber. In the case of *a* the tongue is very considerably lowered in comparison, and also retracted. In other words, the vibrating column of air is now passing through a much wider resonance chamber. This kinesthetic explanation is just as simple as the acoustic one and really means no more than that a spatially extended gesture is symbolic of a larger reference than a spatially restricted gesture. In discussing some of the results with the children themselves, who seemed very much interested in the rationale

of the experiment, the impression was gained that the subjects differed somewhat in the psychological basis of the symbolism, some being apparently swayed entirely by the acoustic factor, others by the acoustic factor only or mainly insofar as it was itself supported by the kinesthetic factor.

The tabulated results, of which we have given a brief sample, have the disadvantage of drowning out significant individual variations. For a preliminary report such a method of presentation is at least suggestive; but it would be important to know to what extent individuals differ significantly in their ability to feel symbolism in sound contrasts. The schedules need to be gone over from the point of view of working out individual indices of 'symbolic sensitiveness' to sounds.

Meanwhile a third experiment, intended to bring out individual idiosyncrasies, was carried out with a number of selected subjects, chiefly adults. The results were interesting.

In this experiment an artificial 'word' was taken as a starting point and assigned an arbitrary meaning by either the investigator or the subject. The subject was asked to hold on to this arbitrary meaning and to try to establish as firm an association as possible between the imaginary word and its given meaning. Some phonetic element in the word, a vowel or a consonant, was then changed and the subject asked to say what difference of meaning seemed naturally to result. The answer was to be spontaneous, unintellectualized. The process was kept on for as long a period as seemed worth while, the saturation point of meaningful and interested responses being reached very soon in some cases, very late in others. In the case of certain individuals more than 50 distinct 'words' were found to build up a constellated system in which the meanings were rather obviously the results of certain intuitively felt symbolic relations between the varied sounds. In the case of other individuals actual word associations tended to creep in, but on the whole there was surprisingly little evidence of this factor. The subjects were found to differ a great deal in their ability to hold on without effort to a constellation once formed and to fit new meanings into it consistently with the symbolisms expressed in previous responses. Some would give identically the same response for a stimulus word that had been—so it was claimed—forgotten as such. In its imaginary, constellated context it evoked a consistent response. Others lost their moorings very rapidly. It is hoped to discuss these interesting variations of sensitivity to sound symbolism, *i.e.* to the potential meaningfulness of relations in sound sets, in the final report of these investigations.

In the present purely preliminary report we can do little more than give a few examples of the responses of two of the subjects, KP and JS. The word *mīla* was arbitrarily defined as 'brook' by the subject KP. Fifty-three responses were obtained from her, starting with this nucleus. The following excerpts from the material will be illuminating.

1. *mila*: "Brook."
2. *mila*: "Smaller brook."

3. *mēla*: "Larger brook; nearer a river; swifter; no longer thought of as part of the meadow landscape."
4. *mēla*: "Larger, not so flowing; large lake like Lake Superior."
5. *mela*: "Little lake for fishing."
6. *mela*: "Smaller brook than *mīla*, larger than *mila*."
7. *māla*: "Larger than brook. Perhaps water running through a ravine; mixed up with the scene."
8. *mīna*: "Water trickling down in a ravine through the rocks scattered on the side."
9. *mēna*: "A little larger but still diminutive. Water travels through a gravel pit."
10. *mēna*: "Deep, narrow, swiftly moving stream rushing through a cut in the rocks."

. . . . . .

14. *mīni*: "Tiny but swift stream spurting out of the rocks like a jet of water."

. . . . . .

18. *mūla*: "Fairly large, rather rambling brook at night."
19. *mōla*: "Ocean at night."
20. *mɔ̄la*: "Ocean in the daytime."
21. *māla*: "Bright ocean."

. . . . . .

2^1. *mila*: (21st response after 2): "A little brook." (The jump back to the earlier response was made at once, without hesitation.)

. . . . . .

14^1. *mīni*: (15th response after 14): "Spurt of water from the rocks, small but swift."

. . . . . .

48. *wīla*: "Can be wet, but water is more like dew on wet grass after rain. Belongs to the same set."
49. *wēla*: "Wet trees after a rain. No feeling of a body of water. General dampness, a 'larger' feeling than *wīla*."

Not all subjects by any means were as responsive as KP; but a surprising number showed a very definite tendency toward the constellating of sound symbolisms. A few responses from JS, based on the same stimulus word, will be interesting for purposes of comparison. The meaning 'brook' was assigned by the investigator and accepted as satisfactory by the subject.

1. *mila*: "Brook."
2. *mēla*: "Seems to sort of broaden out. Brook got much calmer."
3. *mila*: "Got to chattering again; smaller brook; stones visible, which make the noise."

4. *m̄la*: "Brook gets stagnant with rushes growing in it. The rushes hold the water back so it forms pools. The flow is in the middle; relatively stagnant at the edges."
5. *māla*: "Almost like a lake. An uninteresting lake."
6. *māla*: "More color in it. May have been shallow before; now has greater depth of color, greener shadows; still a lake."
7. *mela*: "Pools taken out at the side from 4. Regains a little of its chattering. Sort of tiny. Less cheerful and chirpy than 3. No great difference as to size between 7 and 3. Merely has a deeper note."

. . . . . .

6^1. *māla* (4th response after 6): "Nice broad pool with all nice colors in it. Shadows and water rich green, as of tree shadow in pool."

. . . . . .

1^1. *mīla* (11th response after 1): "Rather nice chattering brook."

. . . . . .

17. *mile*: "A little splash of water. Tiny stream hit a rock and spattered out in all directions."
18. *mīli*: "Water has gone. A bit of rather dense woods with lots of moisture. Water not evident, but obviously somewhere. You don't see water but you know it is there. Rather soggy to walk around."

. . . . . .

6^2. *māla* (23d response after 6^1, with much material in between that was definitely removed from suggestions of 6): "Quick sweep of water view over a lake. Not just a pond. A few islands, but they look like dots. The sun is setting. There are nice black shadows this side of the island. The scenery is darkest where I am. I am interested in the distant brightness."

A comparison of these excerpts from the two schedules shows certain interesting resemblances and differences. Both subjects constellate their responses; but KP does so more rigidly, 'geometrically,' as it were. With JS the underlying 'geometry' of response is enriched by imaginative overtones. Incidentally, it will be observed by the attentive reader, a considerable number of the responses here quoted from the third series of experiments check some of the magnitude symbolisms independently obtained from the first and second. This is true of most of the schedules in this set and is significant because neither magnitude variations nor any other class of variations in the responses had been suggested.

It is believed that studies of this type are of value in showing the tendency of symbolisms to constellate in accord-

ance with an unconscious or intuitive logic which is not necessarily based on experience with the stimuli in their normal, functional aspect. In the realm of articulate sounds, to take a specific type of perceptive field, it is believed that the experiments here referred to give cumulative evidence for the belief that unsocialized symbolisms tend to work themselves out rather definitely, and that the influence of specific, functional language factors need not be invoked to explain these symbolisms.

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MEASURING AUDITORY INTENSIVE THRESHOLDS IN ELECTRICAL UNITS

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In spite of the fact that the audio-oscillator is coming more and more into general use [1] for research in sound, the complication of the apparatus, the difficulty of controlling the duration and time-intervals of the tones produced, and particularly the lack of a standardized unit in which to measure the stimuli, have proved to be serious drawbacks. An attempt at a solution of these problems with reference to the determination of sound intensities is reported in this paper. The set-up described presents a number of unusual features, the most outstanding of which are its relative simplicity and the fact that it can be used for obtaining differential thresholds by each of the principal psychophysical methods, without the necessity of making changes or adjustments. Calibration in units of electromotive force furnishes a basis for the comparison of results with those of investigators using different equipment.

The Apparatus

1. *Oscillator.* The audio-oscillator chosen as the source of sound was of the tuning fork variety [2] described by Halver-

[1] See, *e.g.* H. Fletcher and R. K. Wegel, The frequency sensitivity of normal ears, *Phys. Rev.*, 1922, 19, 553–65; H. M. Halverson, Tonal volume as a function of intensity, *Amer. J. Psychol.*, 1924, 35, 360–367; T. J. Jenkins, Facilitation and inhibition, *Arch. Psychol.*, 1926, no. 86, 1–56; V. O. Knudsen, The sensitivity of the ear to small differences of intensity and frequency, *Phys: Rev.*, 1923, 21, 84–102; F. W. Kranz, Sensitivity of the ear, *ibid.*, 1923, 22, 66–72; J. P. Minton, Physical characteristics of normal and abnormal ears, *ibid.*, 1922, 19, 80–96; F. A. Pattie, An experimental study of fatigue in the auditory mechanism, *Amer. J. Psychol.*, 1927, 38, 39–58; E. G. Wever and S. R. Truman, The course of the auditory threshold in the presence of a tonal background, this Journal, 1928, 11, 98–112.

[2] In contrast to the more complicated and expensive vacuum tube oscillators.

son.[3] The frequency cycle of the ac output was 1000, and consequently the tone heard by the S through the earphones was of 1000 d.v. The source of current was a 6-volt 100-ampere-hour Westinghouse storage battery. A 200-ohm variable resistance (Fig. 1) connected in series with this permitted

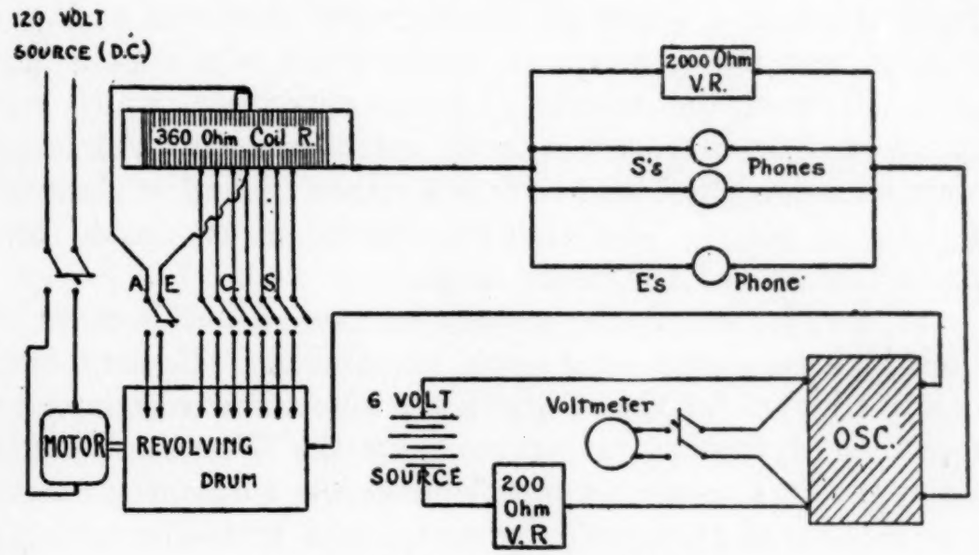

FIG. 1. Schematic diagram of wiring

the cutting down of the input voltage of the oscillator to any desired figure which could then be read from a Weston 7.5-volt range voltmeter (dc) shunted across the input terminals. Best results were obtained when the variable resistance was increased till the voltmeter reading dropped to 3. The oscillator was built to operate on 6 volts, but any tendency for the instrument to produce harmonics was found to be exaggerated when it was running with full input voltage. At half voltage the tone was so pure that no partials could be detected by the observer. Although the presence of harmonics has been shown not to affect intensity and frequency thresholds,[4] it was nevertheless deemed preferable to use a tone approaching as closely as possible the pure sine wave.

Because of the fact that the tuning fork of the oscillator when in operation produced a loud penetrating tone in-

[3] H. M. Halverson, The audio-oscillator, *Amer. J. Psychol.*, 1927, 38, 294–295.

[4] V. O. Knudsen, The sensitivity of the ear to small differences of intensity and frequency, *Phys. Rev.*, 1923, 21, 96.

dependently of that heard in the earphones, considerable attention was given to the construction of a sound-proof box to contain the instrument. When completed this box consisted of three concentric layers. The covering immediately around the oscillator was of ¾-inch soft white pine, the middle layer (allowing a 1-inch air space around the inner one) was also of soft wood, and was closely lined with 2-inch hair felt. Between the middle and the outer box, which was made of sheet-rock, a 3-inch air space was left. All boxes were so constructed that their lids either hooked or clamped tightly in place. The oscillator when in use inside this protection proved absolutely silent.

2. *Earphones.* Radio phones were tried but because of their high resistance were found unsuitable. The earphones finally adopted for the subject's use were standard telephone receivers of 60-ohm rating built by the Western Electric Company.[5] The experimenter's phone was a Stoelting watch-case receiver of 75 ohms.[6] By means of a 2000-ohm variable resistance (connected in parallel with the earphones) the general level of intensity of the series of tones to be compared could be increased or reduced. In any given experiment the 2000-ohm resistance was set at a definite point and left there. It was of use, however, in obtaining the differential threshold at various levels, as, for example, in testing Weber's Law.

3. *Control of the time intervals.* In order to insure a constant duration for both standard and comparison tones and to establish a regular interval of silence between tonal pairs, a 6-inch brass revolving drum driven by a phonograph motor[7] was introduced into the circuit. One lead from the

[5] Dr. H. Fletcher, inventor of the Western Electric Pitch Range Audiometer, suggested these phones which are commercially known as no. 528 BW. The writer gratefully acknowledges other helpful suggestions proffered by Dr. Fletcher during the construction and refinement of the apparatus.

[6] The importance of the earphones and their hook-up cannot be too strongly emphasized. The whole apparatus was virtually built around them. With resistances properly balanced and the apparatus so adjusted that it was functioning perfectly, change to a different type of phone would throw the whole system out of equilibrium.

[7] Double reduction worm gears produced a speed ratio of 900-to-1 between motor and drum. The speed of the drum was so uniform that a time marker run from it in

oscillator was taken to an axle bearing of the drum, and connections were made from the other lead to 9 separate contact points of spring copper which pressed firmly against the drum's circumference. Tight-fitting stencils of heavy paraffine-paper [8] covered the drum; these were perforated at intervals to permit the points to make and break contact with the drum surface.

The holes in the stencil material were grouped into two units and were so arranged that, although the points touched one at a time, contacts by different points were made in successive temporal pairs. The first stencil unit, for use with the Method of Constant Stimuli (C.S. in Fig. 1) contained holes for seven contact points. The holes were so cut that the first member of each temporal pair to touch the drum was always the same point, and the second member was any one of the seven points (including the first itself, when objectively equal pairs were desired). The sound produced by this arrangement followed a pattern similar to, although not exactly the same as, that used in the Seashore phonograph records for pitch and intensity. The duration of each tone of each pair was .5 sec, with an interval of .5 sec between the members of the pair.[9] Between pairs 2.5 sec were allowed.

The second stencil unit (A.E. in Fig. 1), for use either with the Method of Average Error or the Method of Minimal Change, contained holes for two contact points only. One point always made contact before the other in a rhythm, the same as that used in the first unit.

connection with another experiment showed no measurable fraction-of-a-second error after more than 400 seconds, when calibrated against a Jacquet chronograph.

[8] After trying a number of materials for use as drum stencils, among which were expensive cloth drawing papers and tracing cloth, paraffine-paper (sometimes called 'parchment') was eventually used because it was the only material found which would not stretch or shrink with changes in atmospheric humidity.

[9] "Discrimination for successive stimuli always involves the memory element. . . . But for intervals from 1/16 sec to 1 sec there is practically no difference." C. E. Seashore, Measurement of pitch discrimination, *Psychol. Monog.*, 1910, 13, no. 53, 35. Although this statement was originally made with reference to the comparison of tones of different pitch, it applies equally well to intensity comparisons.

4. *Changing the intensities.* The current producing each of the seven comparison tones used in the Method of Constant Stimuli was led through a different length of resistance of a 360-ohm coil rheostat. The current for the standard tone, which sounded first of each pair, went through about half the total resistance of the coil. That for three of the comparison tones (S_1, S_2 and S_3) went through less resistance and the resulting tones were consequently louder or stronger than the standard. That for the remaining three (W_1, W_2 and W_3) went through more resistance and the tones produced were weaker. The order in which the various pairs occurred was varied by changing the connections between specific contact points on the drum and the rheostat contacts to which they corresponded. In using this method (Constant Stimuli) subjects were instructed to judge the second tone of each pair as either 'weak' or 'strong' with reference to the first. Two telegraph keys were adopted as an improvement in the usual procedure of verbal judgments.

With the Method of Average Error and the Method of Minimal Change the second tone of each pair was adjusted by means of a sliding contact on the rheostat. In the Method of Average Error the subject was allowed to listen to as many pairs of tones as he wished, moving the rheostat contact-point after each pair, till he was satisfied that both members of the pair sounded the same.[10] With the Method of Minimal Change the experimenter adjusted the second tone at a constant rate while the subject rendered judgments of equality and inequality. A rheostat scale in half-millimeters enabled accurate readings to be taken *in arbitrary linear units.*

Thresholds in Units of Electromotive Force

1. *Calibration.* In order to find the voltage across the earphones, the following method was employed. The input

[10] This whole section of the apparatus was enclosed in a box protruding from which the subject saw only a small circular brass handle. If he turned this in one direction the second tone of each pair became stronger; if he turned it in the other direction the second tone grew weaker. The connection of the handle to the sliding contact of the coil resistance was made by means of 8-to-1 reduction gears, pulleys and cable.

to an amplifier was measured by determining the alternating current through a known resistance which would give a deflection on the output of the amplifier the same as that produced by the unknown voltage. The current measuring apparatus for the input of the amplifier was then calibrated against direct current. The calibration set-up [11] was arranged as shown in Fig. 2.

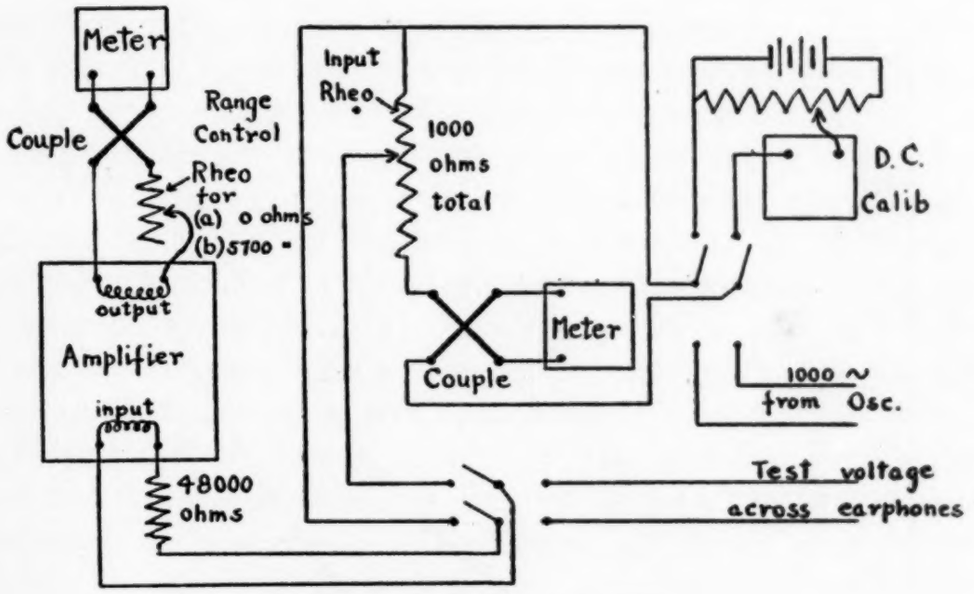

FIG. 2. Calibration apparatus

The relationship between the input voltage to the phones and the scale readings on the 360-ohm rheostat (Fig. 1) within the range of threshold values is indicated in Fig. 3. Curves are presented for two levels of intensities, comparable to dc resistances of (a) 24 and (b) 43 ohms respectively in the 2000-ohm variable resistance (Fig. 1) in parallel with the earphones.[12] Equations of these curves in which Y is expressed explicitly as a function of X were found to be, for (*a*)

$$Y = [(10)^{10}]^{-(.022615X + 1.66082)},$$

for (*b*)

$$Y = [(10)^{10}]^{-(.0424X + 1.0554)},$$

[11] Instruments were loaned by the Physics Department of Columbia University.

[12] The non-linear relationship found between voltage and scale readings may throw some doubt upon the work of several investigators who, in calculating thresholds in terms of arbitrary rheostat scale-settings, have tacitly assumed these to be linearly related to sound intensities.

where for equation (*a*) Y represents millivolts and X linear scale readings in centimeters; for (*b*) Y represents millivolts

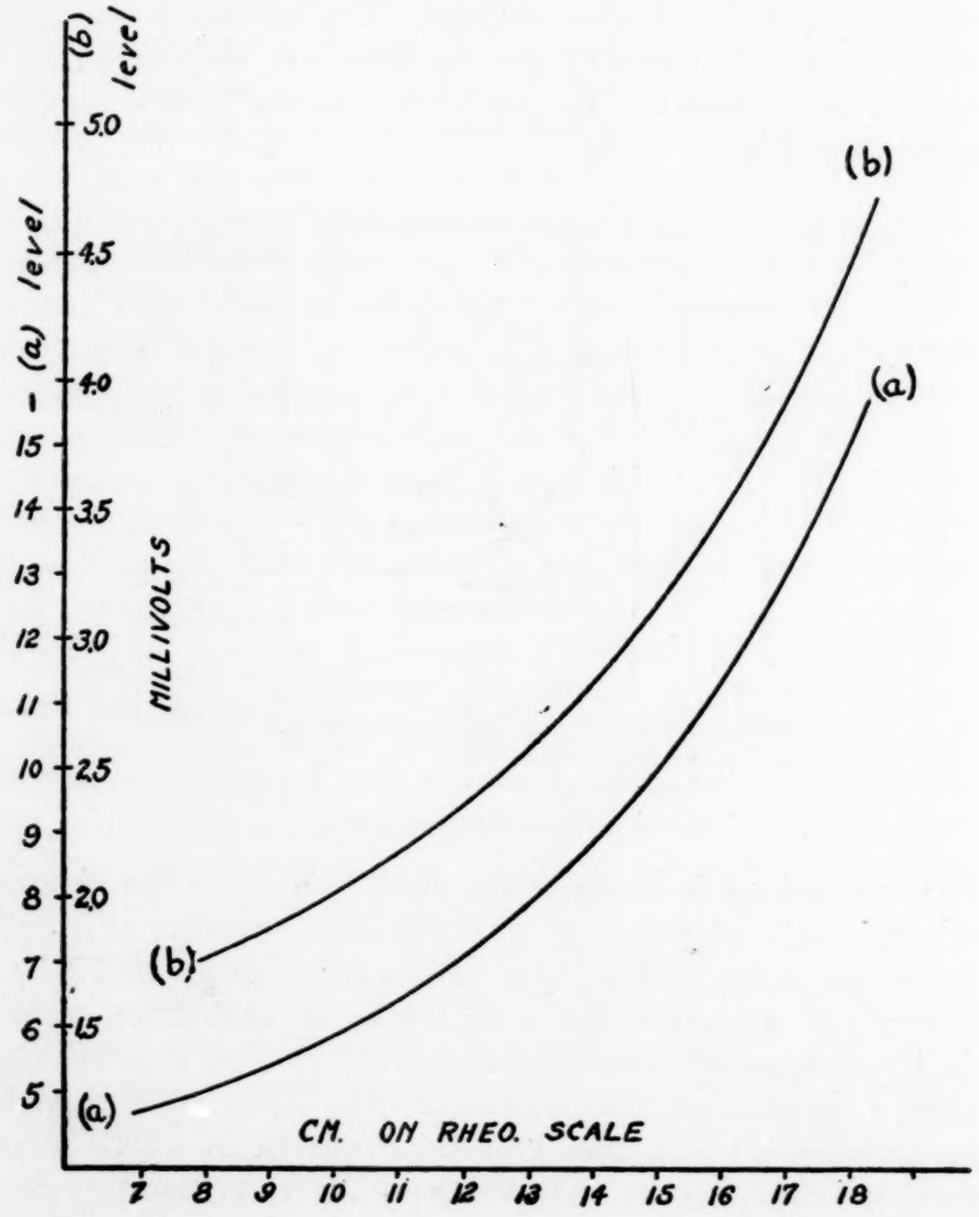

FIG. 3. Relation between rheostat settings and voltage

in units of 10 and X centimeter scale-readings. Arranged in more workable form they become

$$(a) \qquad \log_{10}(\log_{10} Y) = -.022615X - 1.66082,$$

$$(b) \qquad \log_{10}(\log_{10} Y) = -.0424X - 1.0554.$$

By means of these equations, centimeter scale-readings were readily converted into millivolts. Thresholds and individual settings could then be compared in terms of the latter units.

2. *Validity of the method.* The fundamental assumption involved in the calibration procedure is that the intensity of the sound emitted by the earphones is related linearly to the voltage required to produce the sounds. The correctness of the assumption in turn depends on two premises. (1) That the *voltage* is linearly proportional to the *current* making the sound in the earphones.[13] (2) That the *current* (amperage) is linearly related to the *intensity* of the sound it produces.

(1) The first premise is true if the inductance effect in the phones from the ac oscillations is constant. It is highly probable that this is the case within the limited range of intensities employed. (2) Knudsen's [14] work furnishes evidence in support of the second premise. He has shown beyond a doubt that granting Wien's [15] findings on sound intensity and the inverse square law, the intensity of the tone emitted by an earphone is linearly related to the current required to produce it. Dr. Harvey Fletcher also states definitely that "throughout a very wide range of intensities . . . there is a constant ratio between the electrical current supplied to the receiver and the intensity of the sound which it produces when it is held on the ear." [16]

[13] Unfortunately, it was found impossible to measure the current directly without the introduction of indeterminate errors. The infinitesimal amount of this current, being of the order of microamperes, and the necessity of using calibration instruments which possessed at least some resistance, resulted in an unknown distortion of true current readings. With the arrangement finally adopted for measuring the voltage, however, it will be seen that a 48000-ohm resistance (Fig. 2) was inserted in series with the calibration apparatus. This eliminated any possible error resulting from the calibration hook-up.

[14] *Op. cit.*, p. 89.

[15] Max Wien, Ueber die Messung der Tonstärke, *Wied. Ann. der Phys.*, 1888, 36, 834.

[16] Letter to the writer. A more complete excerpt from the letter is as follows. "As far as I know there are no other published papers than the one you mentioned which give any data on the non-linearity of telephone receivers. However, considerable work has been done in our laboratories to determine this point and we feel satisfied that throughout a very wide range of intensities, probably larger than the one you

3. *Threshold values.* Preliminary work with the apparatus indicates that the level of the thresholds lies, for intensity level (a) roughly between .4 and .8 millivolts; for intensity level (b) between 1.1 and 2.2 millivolts. Stated in per cents of the standard stimulus these are, for (a) between 5.6 and 11.2 per cent, for (b) between 4.7 and 9.4 per cent.[17]

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are contemplating using, there is a constant ratio between the electrical current supplied to the receiver and the intensity of the sound which it produces when it is held on the ear. Of course, this only holds when the frequency is held constant. When different frequencies are compared, then the relative currents are no indication at all of the relative intensities produced. Mr. L. J. Sivian of our department has done considerable work along this line and he informs me that he feels quite certain that all of our receivers have a linear relation of this type through most of the ranges of intensities which are ordinarily used."

These statements refer of course to the general relationship between current flow and the sound produced. Intensity is proportional more specifically, however, to the *square* of the amperage and to the *square* of the voltage across the phones than to the crude values of these quantities. An elaboration of this point may be found in the writer's paper, An experimental comparison of psychophysical methods, *Arch. Psychol.*, 1929, No. 106 (section on apparatus).

[17] Knudsen's recent work in this field (*op. cit.*) sets the fraction at 1/10.

(Manuscript received Sept. 12, 1928)

A Multiple Groove Board for Testing Motor Skill

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The Measures of Motor Coördination. In accordance with the present trend of psychological thinking, perhaps it would not be far wrong to say that motor coördination or skill has been regarded in the past as the simple sum-total of speed, precision, and steadiness of movement. Tapping, aiming, tracing and the steadiness test may reveal all the beautiful things they are designed to reveal; but they will never throw light upon such simple performances as driving a nail, riding a bicycle or using a saw. These laboratory analyses of human performance into separate meaningless acts remind one of the cry of the configurationists that there is something 'more' in the total situation; that behavior patterns have motor configurations involving the tonic level of the whole muscular and nervous system and that part-activities have their significance only with respect to the character of the whole. The gradual recognition of this fact is shown, as pointed out by Seashore,[2] in the general tendency of adopting the serial-action technique for the study of motor skill.

Seashore distinguished two main types of apparatus for serial action, the *pursuit* type and the *discrimination* type. All the forms of discrimination apparatus listed in his excellent tabular summary make use either of rhythmic clicks (as in his motor rhythm apparatus), or stimuli cards (as in his serial action discrimeter and Münsterberg motorman apparatus), or typewriter exposures (as in Coover's typewriting mechanism, Hansen typewriter arrangement, and Miles typewriting apparatus), or electric lights (as in Seashore 'psychergograph,' McComas' apparatus, Rexroad multiple-choice, Dunlap 'LVN,' and Dunlap 'Felix' apparatus). In all these the reaction is a chain of unit responses upon a set of finger keys. On the other hand, the 'pursuit' forms of apparatus present upon close analysis four distinct classes according to the specific reactions demanded from the specific stimuli.

(1) *Pursuit as aiming.* This is represented by the McDougall dotting machine and Schuster's modification of it. Here a dotted tape is presented to view or a spiral disc presents holes in paper over a metal disc and the response called for is to dot with a stylus. The response is an intermittent hitting or aiming at a target. Once carried out, there is required no further effort either to help out the missed dot or to bother about the unmissed one. It is fundamentally nothing but tapping at a mark. Closely related to this type of response is that of feeding a revolving arm with square blocks as in the Bogardus fatigue apparatus. The block is to be fed once a second to a definite arm.

(2) *Pursuit as tracing.* This is represented by the Snoddy stabilimeter, where

[1] The writer is indebted to Professor W. R. Miles for criticism. The expenses for the developing of the apparatus were met from the Thomas Welton Stanford Fund for Psychological Research.

[2] R. H. Seashore, Techniques for measuring serial action, this Journal, 1928, 11, 45–55. A complete tabular summary and a bibliography of 35 entries are given. The reader is referred there for all references.

a star-shaped notched path is to be mirror-drawn without touching the sides, and by Patten's coördination apparatus, where two worm-gear cranks placed at right angles are to move brass pointers and a pencil in a hexagonal path. Here, to speak strictly, there is nothing to pursue. It is simple tracing, mediate or immediate, along a prescribed path.

(3) *Pursuit as adjusting.* Miles' pursuitmeter, although called by that name, is in fact not measuring the genuine 'pursuit' response of following a continuously moving stimulus, trying to catch it up and moving in sympathy with it. It aims, on the contrary, at holding the wattage indicator at its neutral position by a compensating current-change through a rheostat lever. The characteristic is that the subject is *adjusting for* the moving stimulus to attain its neutral position in which it is supposed to come to rest. Resting means no more than 'adjusting for.'

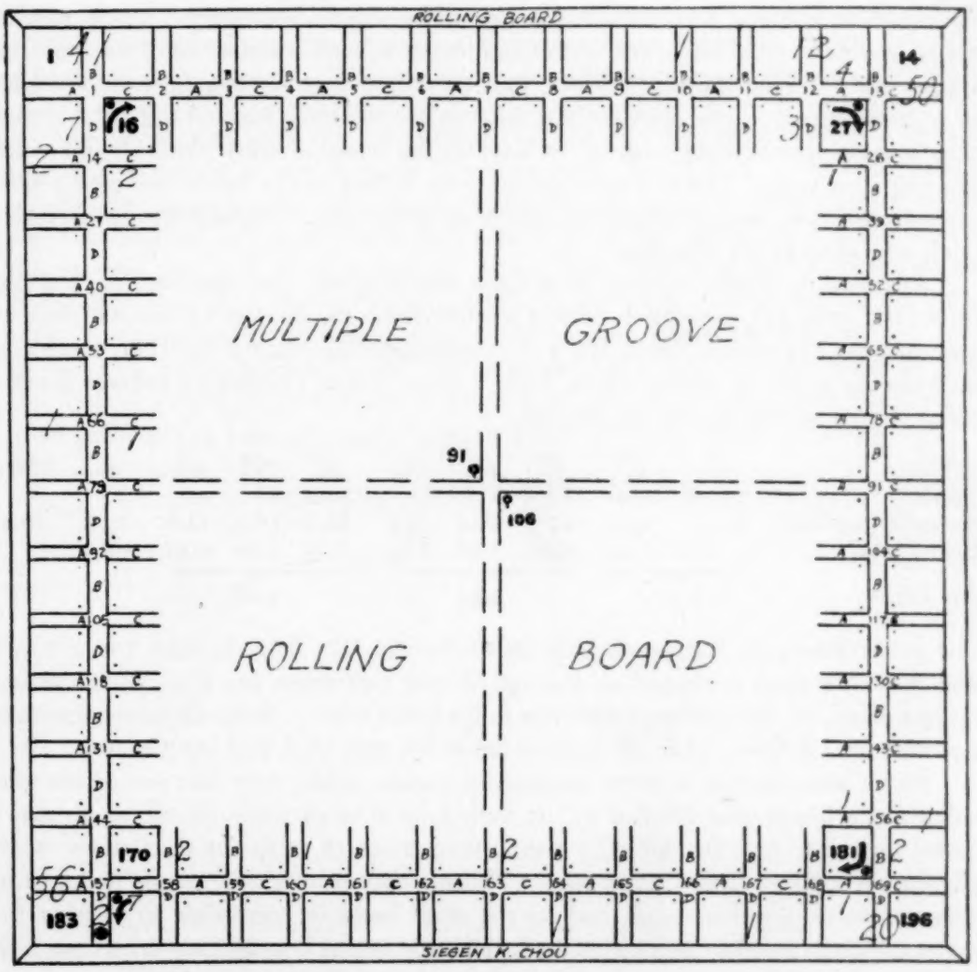
(4) *Pursuit as pursuing or following.* The genuine 'pursuit' response would be to pursue or follow a continuously moving stimulus, either along the arc of a circle as in the Miles pursuit pendulum, or in a circle on a rotating disc (as in Koerth's pursuit rotor), or in a straight line (as in Wishart's pursuit apparatus), or along irregular paths (as in the Renshaw-Weiss and the Schuster pursuitmeters). Here the characteristic is to try to follow the stimulus as closely as possible so as to be able to move in unison with it. The stimulus is the target, in a certain sense; but it is an enduring target, a target to be kept up with.

The above analysis of the stimulus-and-response patterns of the various forms of 'pursuit' apparatus is not so much a criticism of the so-called pursuit tests of serial action as an advance toward the description and introduction of still another technique (the 23d) for measuring serial action.

The multiple groove board. The stimulus-response pattern involved in this new and extremely simple method is essentially the control of the action of a rolling steel ball along definitely prescribed grooves on a multiple groove board by tilting the entire board with two hands. It was when the writer was developing a universal discrete-path 'finger' maze that the idea came to him that, instead of watching a rat or a stylus running along the grooves or alleys of a maze-pattern, the experimenter or observer might just as well run a ball along them. In this case, we are measuring not the intelligence or stupidity of the rolling ball but the skillfulness or clumsiness of the person who is rolling the ball, because all along the grooves there are so many treacherous by-paths that the ball has every chance of rolling into the wrong path, and it is only through constant alertness in eye-hand coördination in adjusting the entire board to compensate the inertia or momentum of the ball that a normal person can satisfy the criterion of three perfect trials. Here the reaction called for is neither that of *aiming*, as in the first case, nor that of *tracing*, as in the second. It is genuine 'pursuit' in the sense that the tilting of the entire board should be in perfect unison with the rolling ball along its true path and this unison should be maintained throughout the entire act. It most resembles the *adjusting* movements in the Miles pursuitmeter, except that the whole trunk and the legs and feet are involved in the adjustment. A better situation, a simpler technique, a stimulus-response pattern more closely related to actual conditions of human performance, could scarcely be found.

A piece of ½-inch soft wood 18 inches square was cut on a disc saw into 13 rows and 13 columns of grooves 5/16-inch wide and ¼-inch deep, so that 196 blocks each 1 inch square were formed. Then four strips of wood were nailed to the four sides to serve both as a reinforcement and as a boundary. The grooves were carefully sandpapered. Both the blocks and groove-crossings were consecutively numbered for

identification. The four arms of each groove-crossing were marked by the letters A, B, C and D in such a way that Bs and Ds in alternate rows occupy all the vertical arms and As and Cs in alternate columns occupy the horizontal arms. On each corner of every block a small hole was drilled. The whole board was divided into four



quadrants along the grooves by a faint green line near the edges of the bottom of the central grooves. A simple square path-pattern was similarly marked by red along the four outermost grooves. One small ring screw was screwed in on each of the opposite corners of blocks 91 and 106. The figures and letters were conveniently done by means of office printing rubber stamps. When everything was done, the board was varnished in its natural color.

The general procedure of experiment is to hold the board at the middle of two opposite sides with two hands (the ring screws at the center of the board are for hooking the entire board to the ceiling by two strings if necessary) and to tilt the entire board in all directions in such unison with the inertia and momentum of a rolling ball (¼-inch bicycle ball bearing) that the ball will roll from a starting point along a prescribed path-pattern to a goal without entering any intersecting side-grooves. Errors may be easily recorded by the aid of the figures and letters at each crossing, while the starting, timing and reaching the goal position are not unlike that of running a rat or stylus in a maze-pattern. The path-pattern may be conveniently marked out by inserting thumb tacks or upholstery nails in the holes on the corner of every block around which the ball is to turn. The rule is to count every rolling of the entire ball into a side-groove or every backward rolling over one block as an error. Three perfect consecutive trials are taken as the criterion.

Preliminary results. Using the marked simple square path-pattern, *i.e.* starting from D of cross 157, turning at cross 1 around block 16, at cross 13 around block 27 and at cross 169 around block 181 and returning to the original position, 9 subjects individually made, at various times, from 8 to 235 trials, yielding an average median speed of 19.4 sec for completing the roll.

Subjects	1K	2C	3F	4K	5L	6P	7H	8S	9C	Mean
Trials	235	144	88	83	80	38	21	21	8	
Seconds (median)	12.2	15.3	15.5	26.4	19.1	22.5	18.9	14.8	29.5	19.4
Error/Trial	1.68	1.95	2.30	3.20	1.83	3.30	3.30	4.43	7.63	
Av. Error	1.37		2.44			3.68				

The susceptibility to improvement is shown by the fact that the first two subjects who made the most trials had an average of only 1.37 errors per trial, while the last subject made, on the average, 7.63 errors in his first 8 trials. Subj. 1K once completed a perfect trial in 6 sec, while 4K took 60 sec in his 39th trial, making 7 errors.

As for the criterion of three consecutive perfect trials, only four out of the nine voluntary subjects ever fulfilled it. It took from 6 to 42 trials for 1K to do this 8 times; from 4 to 35 trials for 2C 7 times; from 22 to 58 trials for 5L 2 times; while 4K did not fulfill it until his 73d trial. This experiment or game, as you may call it, appeared so challenging to 4K and on the other hand so impossible to 9C that the writer could not help but agree with Rockwell [3] that genius is something more than I. Q.

It is certainly a rare experience when, after failing once or twice to obtain the third consecutive perfect trial, the subject makes up his mind once more to start all over again and orientates attentively to the signal of the experimenter and the task before him, only to find his ball hesitating before the first turn, bumping into the end boundary of the second, meandering far into the inner grooves after the third, and reaching the goal position by turning around the fourth with more mistakes than before. Were psychologists in quest of something simple and cheap, without minding its being too interesting and undignified, they might include this multiple-groove *Rolling Board* in their laboratory equipment!

The total 222 errors of the first 20 trials of Subj. 1K, 2C and 7H were located in the figure at their respective places. The ball tends to bump into the end-alley 157A most frequently after the third turn, although the second end-alley 13C, opposite the goal position, is only a little less frequently entered. The third turn is usually much easier than the first.

[3] J. G. Rockwell, Genius and the I. Q., *Psychol. Rev.*, 1927, 34, 377–384.

Summary. The '*pursuit*' type of apparatus for serial action as listed by Seashore may be divided into four classes depending upon the pattern called for; pursuit as *aiming*, as *tracing*, as *adjusting* and as *pursuing* or *following*. The stimulus-response pattern involved in the new, simple and cheap apparatus called the multiple-groove *Rolling Board* is essentially designed to control the action of a rolling ball along definite, variable, prescribed groove paths on a multiple-groove board by tilting the entire board with the two hands. The reaction is considered to be genuine 'pursuit.'[4]

[4] In connection with the development of this board, a simple method for constructing a groove or stylus maze was suggested. It consists of nailing pieces of wooden blocks on a panel base along all the boundaries of the maze-pattern so that walls are formed by the blocks on each side of the path. Miles has actually adopted this method (see J. R. Knotts & W. R. Miles, Notes on the history and construction of the stylus maze, *J. Genet. Psychol.*, 1928, 35, 415–427) for constructing the groove maze in connection with a comparative study between the stylus and the finger maze. In fact this multiple groove board can be turned either into a groove maze by filling the unused grooves with suitable strips of wood or into a finger maze by filling the used grooves with specially made strips of wood with a narrow round ridge.

(Manuscript received October 5, 1928)

The Chromopathometer

BY WILLIAM E. WALTON AND BEULAH M. MORRISON[1]

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The chromopathometer is an apparatus for determining the affective values of colored lights by the method of paired exposures. It includes a device for equating intensities, either in pairs or a combination of pairs of colored lights. Colored bulbs in red, blue, green, amber (yellow), and clear (white) were used in the development of this apparatus, which has proved very satisfactory for laboratory demonstrations of the method of paired exposures, of the negative after-image, and of the three variables hue, brightness and saturation.

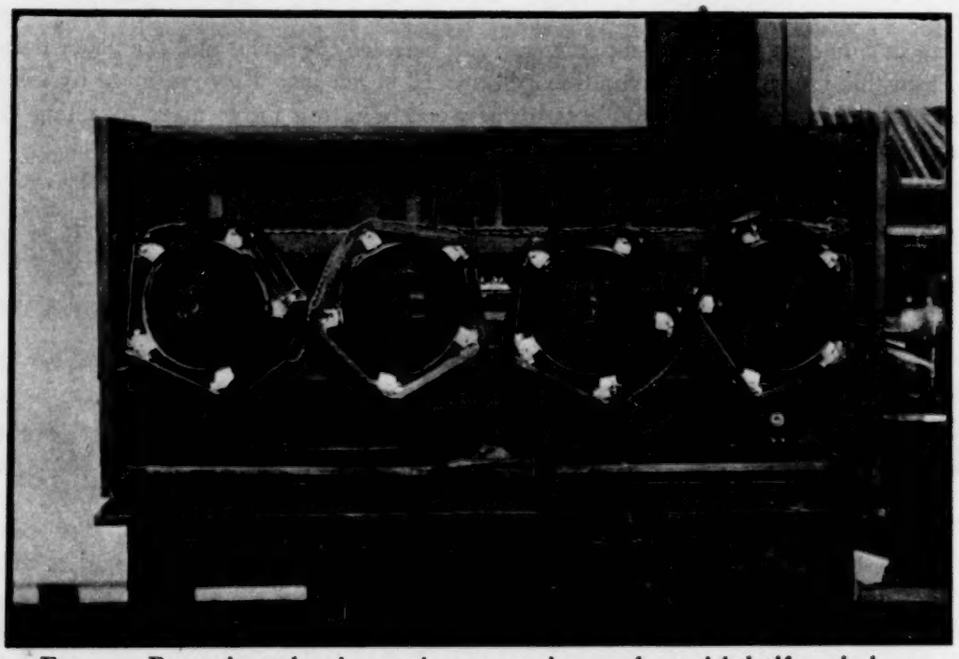

Fig. 1. Rear view, showing projectors, resistance box with knife switch opened, plug and fuse box and cord rack

The chromopathometer must be used in a semi-dark room in order that the lights may be clear and saturated. Enough light may be admitted to enable the observers to record their choices on systematically arranged record sheets from which the results can be easily summarized. The observers should be seated directly in front of the apparatus at least fifteen feet away. When the desired combination of lights has been arranged by means of the small switches of the switch-board, a ready signal is given, and immediately the lights are turned on at the main switch (Fig. 2) for about ten seconds. In a simple demonstration of the method of paired exposures the total time required for single color comparisons is about ten minutes, and for combinations about forty-five minutes.

The apparatus consists of four projectors situated behind a screen in which there is a window of frosted glass for each projector. Also behind the screen there are a

[1] The apparatus was designed and constructed by Walton under the direction of Morrison.

resistance box, a fuse and plug box and a switch board. Fig. 3 shows diagrammatically the complete assemblage and the adjustment of a projector in its frame. Fig. 4 is a working drawing of a single projector with its wiring and screen. The screen is medium gray and the rest of the apparatus black. Each projector unit consists of a lens, frustum of a sheet metal cone, a metal band, a socket rack, the reflector and its shield.

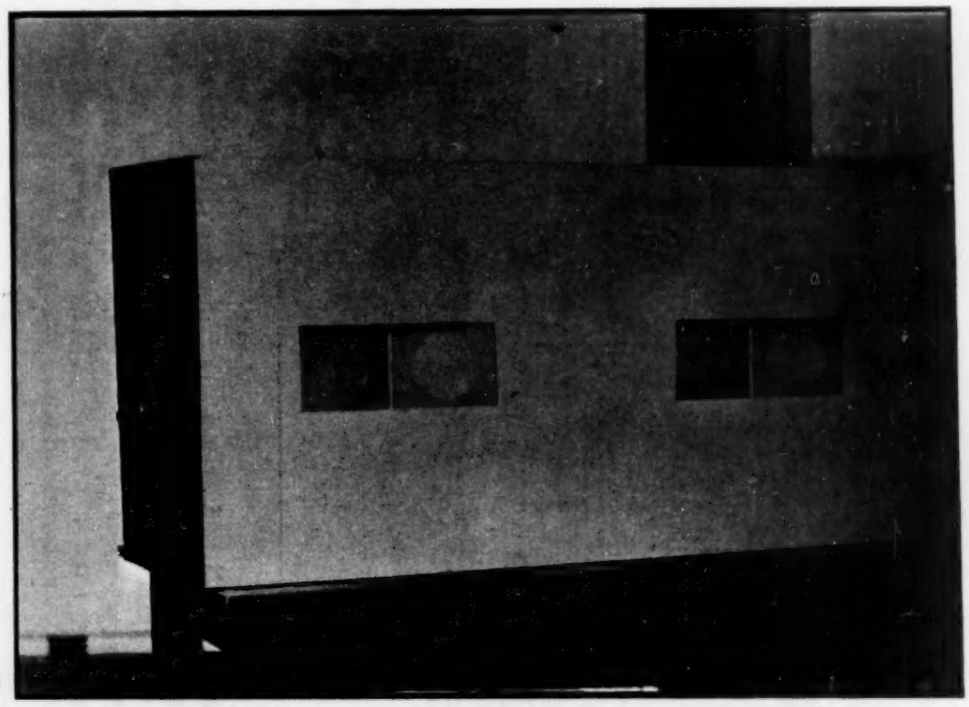

FIG. 2. Front view, showing frosted glass windows and switch-board

The reflector shields were made from ordinary metal shades such as are used over electric lights. The reflector itself is merely a blown glass reflector such as those used on kerosene wall lamps. It is fastened into the shield by two metal strips. The shield holding the reflector is fastened to one side of a band of sheet metal which is five inches wide and has five openings through which the light bulbs are inserted and screwed into the sockets on the socket rack (Fig. 4). The other side of the metal band is attached to the lower base of the frustum of the cone of sheet metal. The lens which is fitted over the upper end of the frustum is held in place by three metal strips. The socket rack is constructed of wooden strips and encircles the metal band in such a way that it allows the sockets to touch the edges of the openings in that band (Figs. 1 and 4). In each projector there is inserted a 40-watt bulb of each of the five colors.

A slight lack of diffusion in the light can be remedied by the insertion in the projector of one or more milk glass screens, which necessitates an increase in wattage.

The four projectors are supported by racks made of wooden strips running lengthwise of the entire apparatus. These racks are shaped to fit the curvature of the frustums and are held in place by cross bars secured to the framework of the apparatus (Fig. 3, end view, 15). The front and sides of this framework are covered with fibreboard with the exception of the frosted glass portions. The framework of the upper parts of the frustums holds the pieces of glass in position (Fig. 3, top view, 10). The whole is placed slightly diagonal to the screen in order to have the two colors close

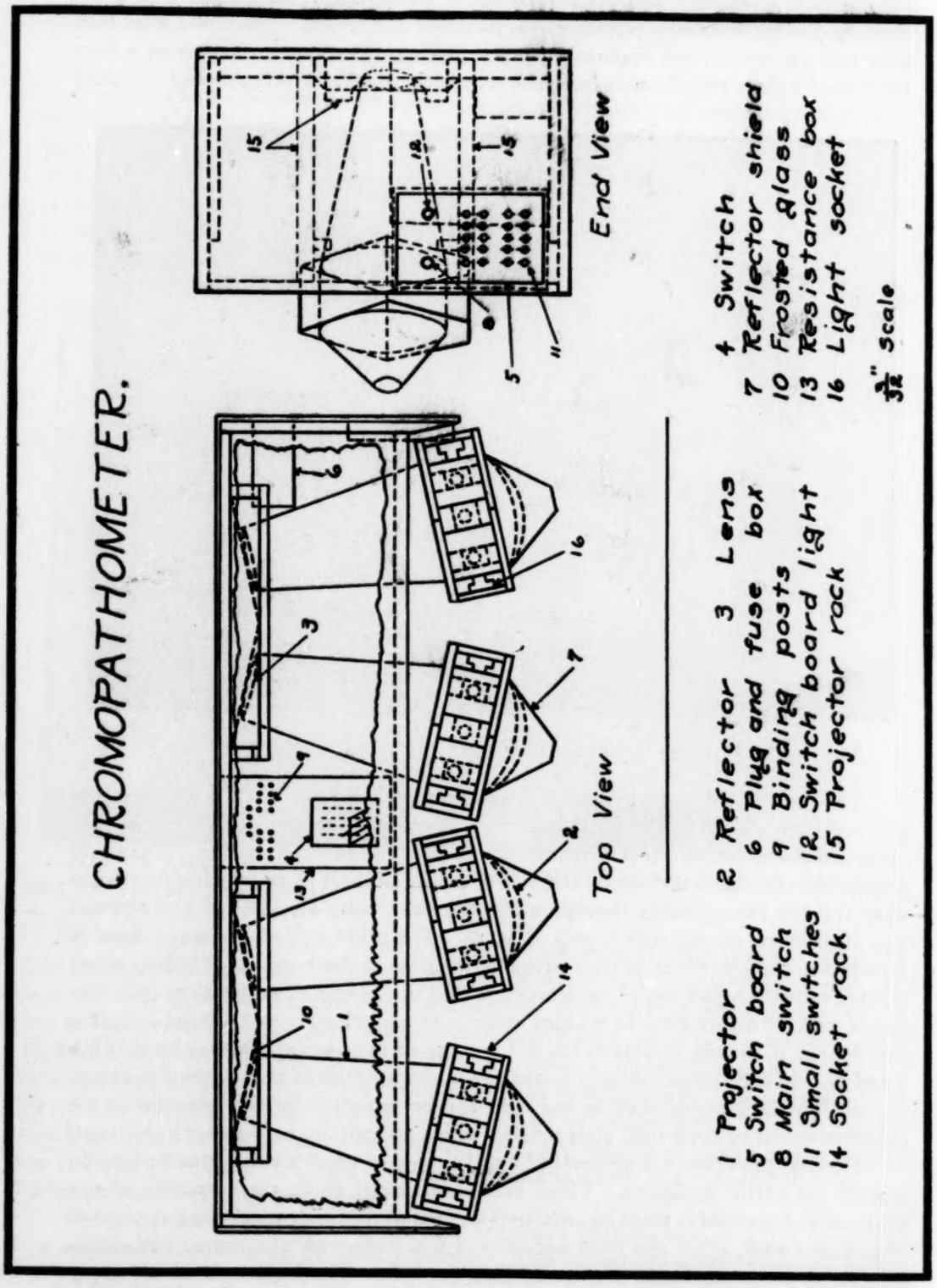
CHROMOPATHOMETER.
End View
Top View
1 Projector
2 Reflector
3 Lens
4 Switch
5 Switch board
6 Plug and fuse box
7 Reflector shield
8 Main switch
9 Binding posts
10 Frosted glass
11 Small switches
12 Switch board light
13 Resistance box
14 Socket rack
15 Projector rack
16 Light socket
1/2" scale

FIG. 3

together and the two combinations of color a greater distance apart in a relatively compact apparatus.

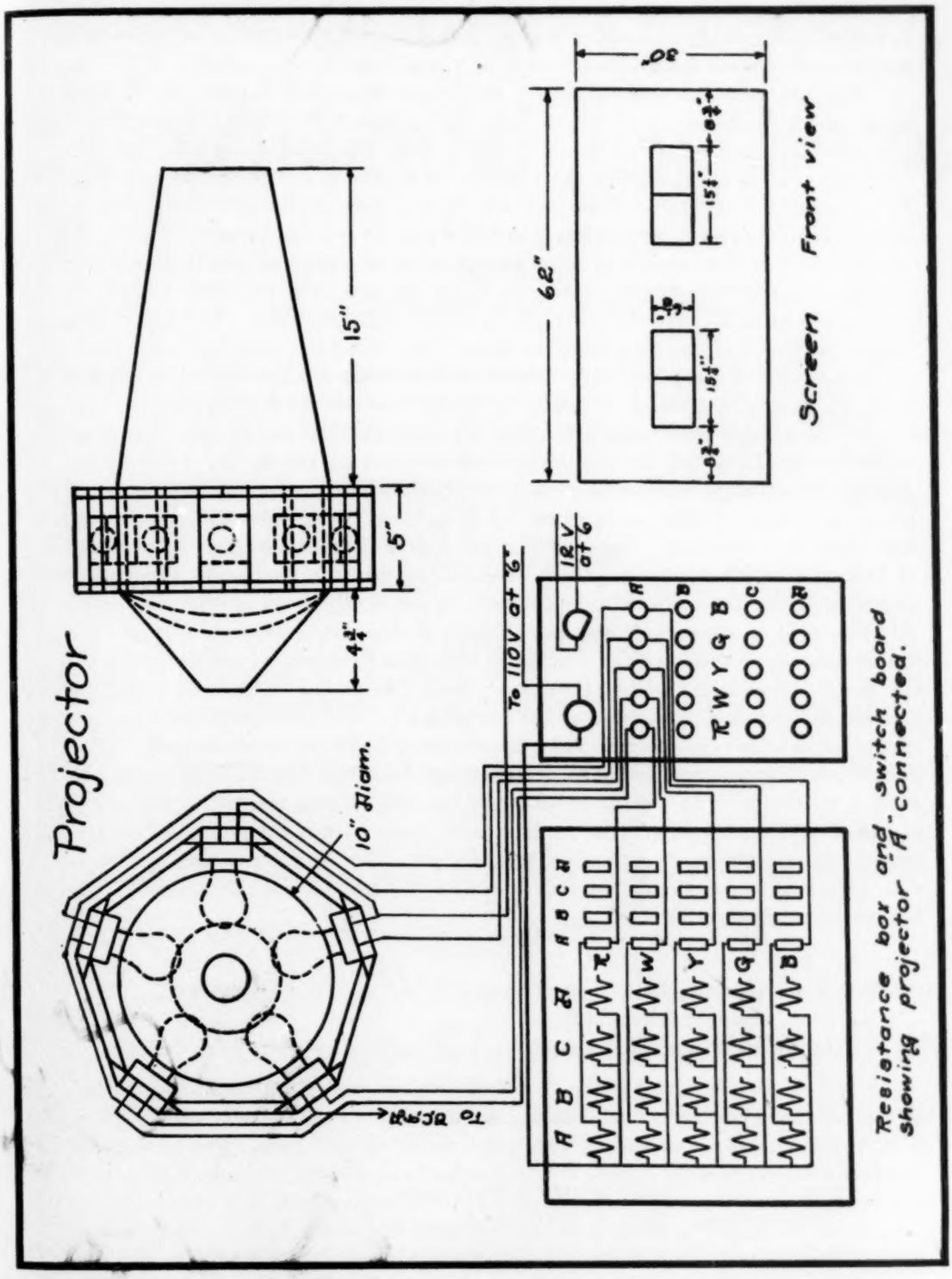

FIG. 4

The switchboard includes the main switch and the small contact switches, one for each of the twenty lights. A small bulb shielded on three sides by a metal cap

FIG. 3

lights the switchboard from the upper right hand corner (Fig. 3, end view, 5, 11, 8, 12).

The plug-and-fuse box contains a fuse and two connections for outside leads of 110-volts and 12-volts of ac or dc current to supply the main part of the apparatus and the switchboard light, respectively (Fig. 3, top view, 6).

The resistance box encloses twenty resistance coils, one for each light. A large knife switch on the top of the box permits the current to be shunted either through the coils when opened (to equate intensities), or to the lights directly when closed (Fig. 3, top view, 13). Binding posts permit the resistance box to be removed easily for adjustment of the coils. These coils are made of chromo-silicon-steel wire, size 30, wound first like a screen door spring, then pulled slightly apart and wrapped on wooden rollers. Thus it was possible to insert 2892.02 ohms of resistance (required to equate the intensities of these twenty lights) by small, compact coils, allowing at the same time ample space between the wires for insulation and ventilation. The rollers were dipped in wax to fix the wire firmly in place. The wire heats very little under continued use since it has a low heat coefficient. The rollers are fastened to an asbestos board by metal plates which are connected with wires from the binding posts.

A flicker-photometer manufactured by the Franz-Schmidt and Haensch Company of Berlin (no. LDRGM–235910) was used in determining the amount of resistance necessary to equate the intensities. A lamp issued by the Bureau of Standards which produced a light of 3.93 candle power when operated by 80 volts and .34 amperes was taken as a standard. The four blue bulbs were compared to find the one bulb of lowest intensity which was then measured against the standard. The results showed that the blue bulb of lowest intensity had a candle power of .546. By using this blue bulb as a standard, one bulb of each of the other colors was standardized for its color by determining the amount of resistance necessary to produce an intensity equal to that of the original blue. Our method of procedure here was to set the carriage of the flicker-photometer at the half-way mark and then to change the resistance of the unknown light by means of a rheostat until the flicker was eliminated. Some difficulty was encountered here, for in working with different colors the flicker cannot be entirely eliminated. In making these determinations at least ten readings for each of two observers were used. The readings were taken from a voltmeter and an ammeter. After establishing a standard light for each color, it was a simple matter to find the amount of resistance needed to equate the remainder of the lights by the following formula;

$$\frac{R = 110 - V}{I},$$

where R = resistance in ohms,
V = voltage of unknown bulb,
I = amperage of unknown bulb.

Fig. 4 shows the wiring of the chromopathometer with one projector connected to the resistance box and switch-board. A wire leading from the plug-and-fuse box (Fig. 3, top view, 6) is connected with one terminal of each bulb. From the other terminal a wire carries the current to the switch-board, through the individual switch to a connection on the knife switch (shown as small rectangles A, B, C, D on Fig. 4) through its respective resistance coil to the main line which in turn conducts the current through the main switch to the plug-and-fuse box. The letters A, B, C, D of Fig. 4 correspond to the four projectors which are lettered left to right from the rear of the apparatus. The colors are indicated by the letters R, W, Y, G, B.

(Manuscript received January 8, 1929)